PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From May 1, 1890, to December 1, 1890.

VOL. XLVIII.

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CONTENTS.

VOL. XLVIII.

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THE ROYAL SOCIETY.

May 1, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Baker, Sir Benjamin, M.Inst.C.E.
Bosanquet, Robert Holford Macdowall, M.A.
Burbury, Samuel Hawkesley, M.A.
Gardiner, Walter, M.A.
Kerr, John, LL.D.
Lea, Arthur Sheridan, D.Sc.
MacMahon, Percy Alexander, Major R.A.
Norman, Rev. Alfred Merle, M.A.
Perkin, Professor William Henry, jun., F.C.S.
Pickering, Professor Spencer Umfreville, M.A.
Roberts, Isaac, F.R.A.S.
Sharp, David, M.B.
Teall, J. J. Harris, M.A.
Thorne, Richard Thorne, M.D.
Weldon, Walter Frank Raphael, M.A.

The following Papers were read:—

I. "Magnetic Properties of Alloys of Nickel and Iron." By J. HOPKINSON, D.Sc., F.R.S. Received April 17, 1890.

Eight different alloys have been examined, distinguished here by the letters of the alphabet. All the samples were given to me by Mr. Riley, of the Steel Company of Scotland, who also furnished me with the analysis given with the account of the experiments with each sample.

The methods of experiment were the same as were detailed in my paper on "Magnetic and other Physical Properties of Iron at a High Temperature." The dimensions of the samples were also the same. For this reason it is unnecessary to recapitulate the methods adopted. I confine myself to a statement of the several results, dealing with each sample in succession.

A. The following is the analysis of this sample :—

Fe.	Ni.	C.	Mn.	S.	P.	
97·96	0·97	0·42	0·58	0·03	0·04	per cent.

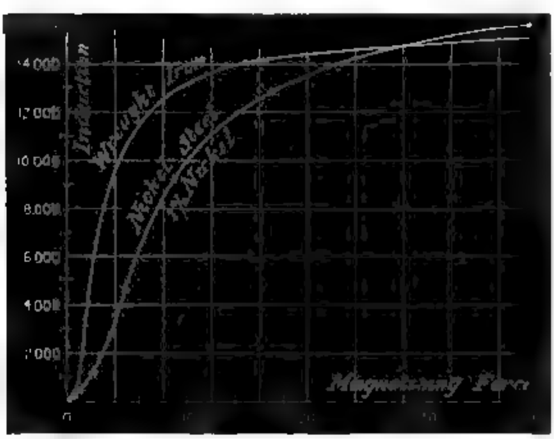
In this case a magnetisation curve is all that I have obtained free from doubt; the sample was heated and its magnetisation determined at various temperatures for a force of 0·50, but the higher temperatures must be taken as a shade doubtful, as the secondary broke down before cooling, and I cannot be sure whether or not the resistance of the secondary may have changed.

Table I gives the results at the ordinary temperature for the material before heating; these are plotted in Curve 1 together with the curve for wrought iron, for comparison.

Table I.

Magnetising force.		Induction.
0·06	......	11
0·12	......	29
0·26	......	58
0·53	......	122
1·07	......	303
2·14	......	995
4·7	......	4,560
8·8	......	9,151
16·8	......	12,876
38·9	......	15,651
270·0	......	21,645

Curve 1.

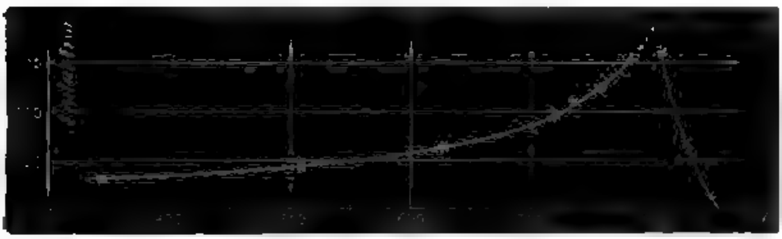


The only noteworthy features are that the coercive force is obviously somewhat considerable, and that the maximum induction is great—greater than that of the more nearly pure iron.

In Curve 2 are shown the results of induction in terms of the temperature for a force of 0·50.

CURVE 2.

1 per cent. Nickel. Magnetising Force, 0·50.



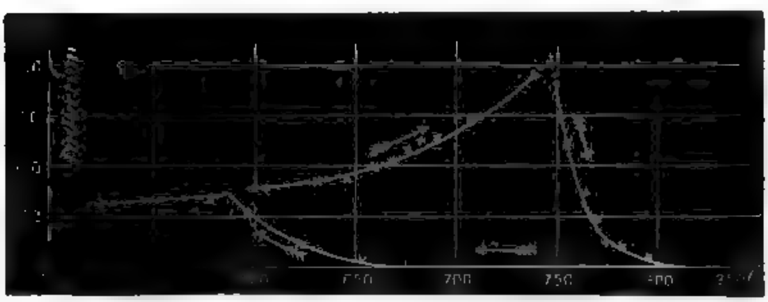
B. The following is the analysis of the sample:—

Fe.	Ni.	C.	Mn.	S.	P.	Si.	
94·799	4·7	0·22	0·23	0·014	0·037	trace	per cent.

We have here results of induction in terms of temperature for a magnetising force of 0·12, shown in Curve 3, and for comparison

CURVE 3.

4·7 per cent. Nickel. Magnetising Force, 0·12.



therewith the results of rate of heating and cooling in Curves 4 and 5 respectively. The experiment with rising temperature was made by simply observing with a watch the hour at which the temperature attained successive values whilst the piece was in the furnace; the cooling experiments were made in exactly the way described in 'Phil. Trans.,' A, 1889, p. 463; in the experiment with rising temperature, however (Curve 4), the ordinates are the actual temperatures, not

CURVE 4.

the logarithms of the excess of temperature above the room, as in Curve 5. The most remarkable feature in Curve 3 is that the material has two critical temperatures, one at which it ceases to be magnetisable with increase of temperature, the other, and lower, at which it again becomes magnetisable as the temperatures fall, and that these temperatures differ by about 150° C. Between these temperatures, then, the material can exist in either of two states—a magnetisable and a non-magnetisable. Note, further, that the curve for decreasing temperature returns into that for increasing temperature, and does not attain to the high value reached when the temperature is increasing. From Curve 4 we see that there is absorption of heat about 750° C., and not before; and from Curve 5 that heat is given off at 632° C., and again at a lower temperature. Comparing these temperatures with Curve 3, it is apparent that the

CURVE 5.

absorption and liberation of heat occur at the same temperature as the loss and return of the capacity for magnetism. From Curve 5 also we may infer that the latent heat liberated in cooling is about 150 times the heat liberated when the temperature of the material falls 1° C. Concerning the latent heat absorbed in heating, nothing can be inferred from Curve 4, excepting the temperature at which it is absorbed.

C. This alloy is very similar to the last; its analysis is—

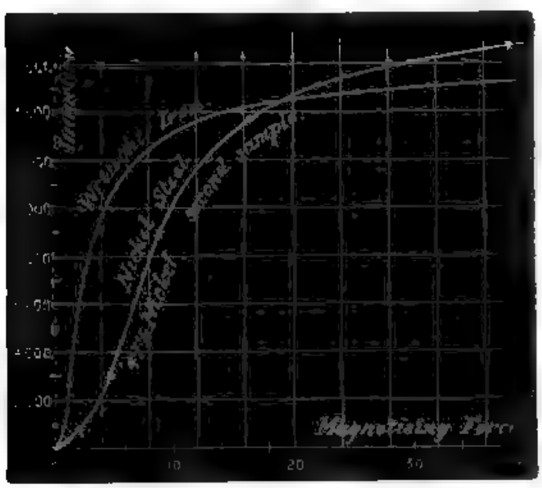
Fe.	Ni.	C.	Mn.	S.	P.	
94·39	4·7	0·27	0·57	0·03	0·04	per cent.

In Table II are given the results of observations of induction in terms of magnetising force at the ordinary temperature of the room; and in curve 6 these are plotted together with the curve for wrought iron.

Table II.

Magnetising force.		Induction.
0·06	......	14
0·12	......	29
0·25	......	60
0·52	......	127
1·05	......	294
2·10	......	760
4·6	......	3,068
8·7	......	8,786
16·6	......	13,641
38·5	......	16,702
266·5	......	21,697

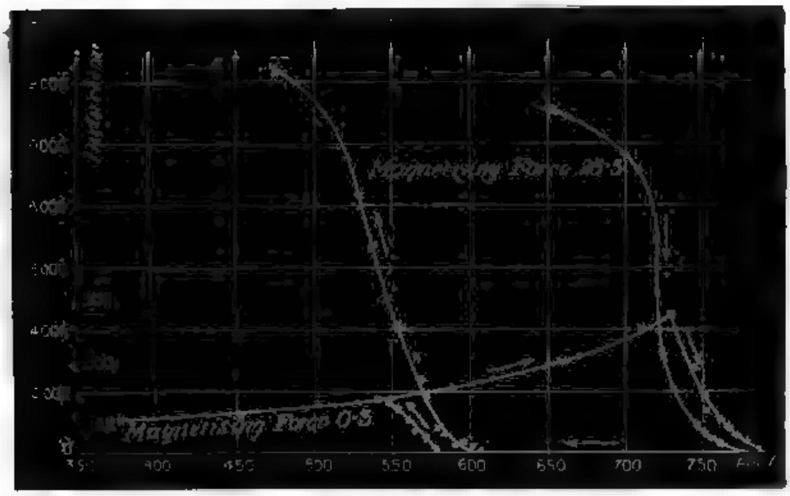
Curve 6.



The material appears to be capable of considerably higher magnetisation than wrought iron. In Curve 7 is shown the relation of induction and temperature for two forces, 26·5 and 0·5, the results being obtained on two different days, to the same scale of abscissæ but different scales of ordinates. These curves show the same features as the alloy B, but at a rather lower temperature.

D. This sample contains 22 per cent. of nickel. It was not thoroughly tested, as the supply of CO_2 which happened to be available was insufficient. Its magnetic properties, however, were similar to the next sample.

Curve 7.



E. The analysis of this sample* was—

Fe.	Ni.	C.	Mn.	S.	P.	Si.	
74·31	24·5	0·27	0·85	0·01	0·04	0·02	per cent.

As the material was given to me it was non-magnetisable at ordinary temperature; that is to say, the permeability was small, about 1·4, and the induction was precisely proportional to the magnetising force. The ring on being heated remained non-magnetisable up to 700° C. or 800° C. A block of the material did not recalesce on being heated to a high temperature and being allowed to cool.

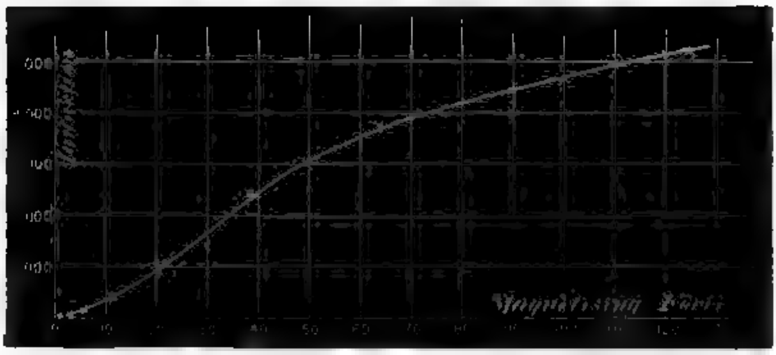
On being placed in a freezing mixture, the material became magnetic at a temperature a little below freezing point.

The material was next cooled to a temperature of about −51° C. by means of solid carbonic acid. After the temperature had returned to 13° C. the curve of magnetisation was ascertained as shown in Curve 8; from this it will be seen that the ring of the material which was previously non-magnetisable at 13° C. is now decidedly magnetisable at the same temperature. On heating the material, it remained magnetisable until it reached a temperature of 580° C. At this temperature it became non-magnetisable, and, on cooling, remained non-magnetisable at the ordinary temperature of the room. Curve 9 shows the induction at various temperatures for a magnetis-

* The results with this sample have already been presented to the Royal Society ('Proceedings,' vol. 47, pp. 23 and 169), but are repeated now for completeness.

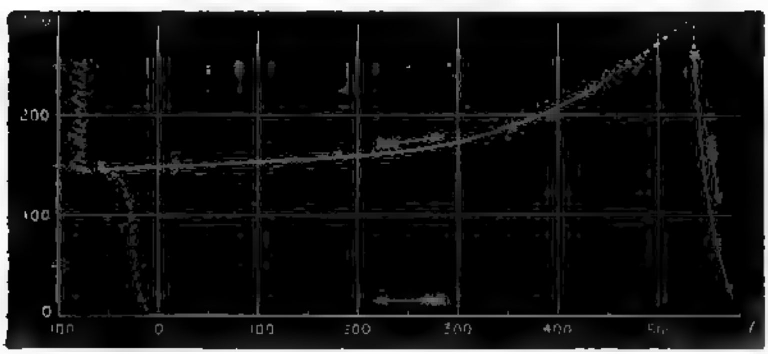
CURVE 8.

25 per cent. Nickel.



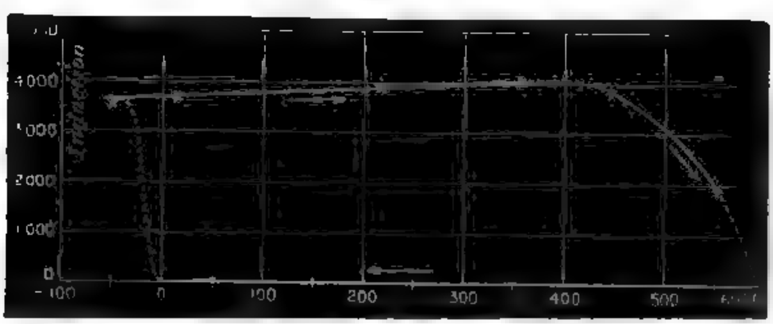
CURVE 9.

Magnetising Force, 6·7. 25 per cent. Nickel.



CURVE 10.

Magnetising Force, 64.



ing force 6·7; whilst Curve 10 shows the induction in terms of the temperature to a different scale for a force of 64. These curves show that, through a range of temperature from somewhat below freezing to 580° C., this material exists in two states, either being quite stable, the one being non-magnetisable, the other magnetisable. It changes from non-magnetisable to magnetisable if the temperature be reduced a little below freezing; the magnetisable state of the material does not change from magnetisable to non-magnetisable until the temperature is raised to 580° C.

The same kind of thing can be seen in a much less degree with ordinary steel. Over a small range this can exist in two states; but in changing its state from non-magnetisable to magnetisable a considerable amount of heat is liberated, which causes rise of temperature in the steel. It is observed in samples B and C of nickel steel, as we have just seen, but at a higher temperature.

As might be expected, the other physical properties of this material change with its magnetic properties. Mr. Riley has kindly supplied me with wire.

The wire as sent to me was magnetisable as tested by means of a magnet in the ordinary way. On heating it to a dull redness it became non-magnetisable, whether it was cooled slowly or exceedingly rapidly, by plunging it into cold water. A quantity of the wire was brought into the non-magnetisable state by heating it and allowing it to cool. The electric resistance of a portion of this wire, about 5 metres in length, was ascertained in terms of the temperature; it was first of all tried at the ordinary temperature, and then at temperatures up to 340° C. The specific resistances at these temperatures are indicated in Curve 11 by the numbers 1, 2, 3. The wire was then cooled by means of solid carbonic acid. The supposed course of change of resistance is indicated by the dotted line on the curve; the actual

CURVE 11.

observations of resistance, however, are indicated by the crosses in the neighbourhood of the letter A on the curve. The wire was then allowed to return to the temperature of the room, and was subsequently heated, the actual observations being shown by crosses on the lower branches of the curve, the heating was continued to a temperature of 680° C., and the metal was then allowed to cool, the actual observations being still shown by crosses. From this curve it will be seen that in the two states of the metal (magnetisable and non-magnetisable) the resistances at ordinary temperatures are quite different. The specific resistance in the magnetisable condition is about 0·000052; in the non-magnetisable condition it is about 0·000072. The curve of resistance in terms of the temperature of the material in the magnetisable condition has a close resemblance to that of soft iron, excepting that the coefficient of variation is much smaller, as, indeed, one would expect in the case of an alloy; at 20° C. the coefficient is about 0·00132; just below 600° C. it is about 0 0040, and above 600° C. it has fallen to a value less than that which it had at 20° C. The change in electrical resistance effected by cooling is almost as remarkable as the change in the magnetic properties.

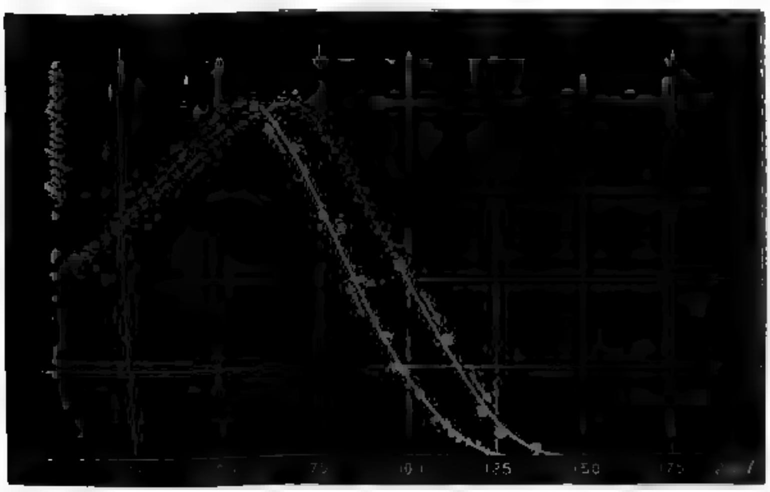
Samples of the wire were next tested in Professor Kennedy's laboratory for mechanical strength. Five samples of the wire were taken which had been heated and were in the non-magnetisable state, and five which had been cooled and were in the magnetisable state. There was a marked difference in the hardness of these two samples; the non-magnetisable was extremely soft, and the magnetisable tolerably hard. Of the five non-magnetisable samples the highest breaking stress was 50·52 tons per square inch, the lowest 48·75; the greatest extension was 33 per cent., the lowest 30 per cent. Of the magnetisable samples, the highest breaking stress was 88·12 tons per square inch, the lowest 85·76; the highest extension was 8·33, the lowest 6·70. The broken fragments, both of the wire which had originally been magnetisable and that which had been non-magnetisable, were now found to be magnetisable. If this material could be produced at a lower cost, these facts would have a very important bearing. As a mild steel, the non-magnetisable material is very fine, having so high a breaking stress for so great an elongation at rupture. Suppose it were used for any purpose for which a mild steel is suitable on account of this considerable elongation at rupture, if exposed to a sharp frost its properties would be completely changed —it would become essentially a hard steel, and it would remain a hard steel until it had actually been heated to a temperature of 600° C.

F. This sample contains 30 per cent. of nickel. Curve 12 shows the relation of induction to magnetising force at the ordinary temperature, and Curve 13 the relation of induction and temperature for

CURVE 12.

30 per cent. Nickel.

CURVE 13.



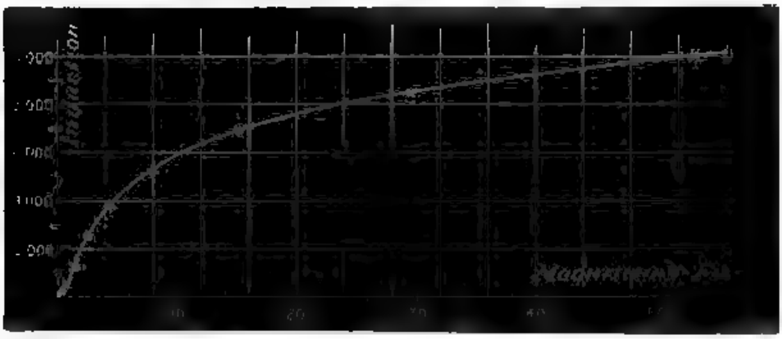
a force of 0·65. The remarkable feature here is the low temperature at which the change between magnetisable and non-magnetisable occurs, whether the temperature is rising or falling. Comparing it with the last sample, we see that the character of the material with regard to magnetism is entirely changed.

G. The analysis of this sample is—

Fe.	Ni.	C.	Mn.	S.	P.	
66·19	33·0	0·28	0·50	0·01	0·02	per cent.

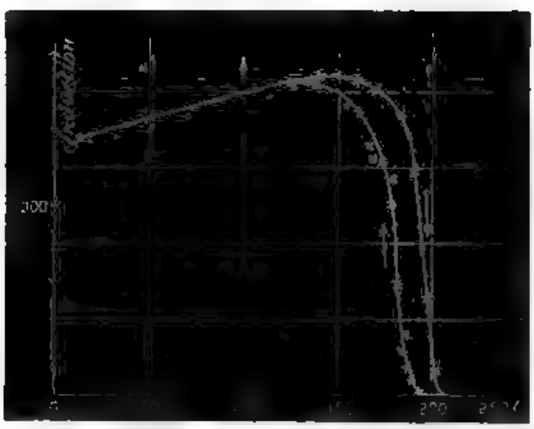
Curve 14.

33 per cent. Nickel.



Curve 15.

Magnetising Force, 1·0.



In Curve 14 is given the relation of induction and force at the ordinary temperature, and in Curves 15 and 16 the relation of induction and temperature for forces 1·0 and 30·3. The remarkable feature of this material is the complete difference from the last but one, and the low temperature of change. There is but very little difference between the temperatures of change when heated and when cooled.

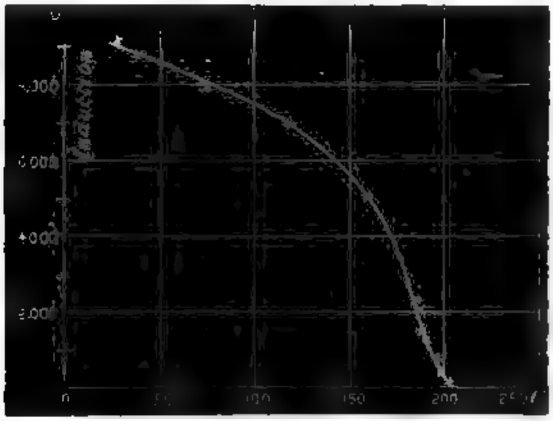
H. The analysis of this sample, as furnished by Mr. Riley, is—

Fe.	Ni.	C.	Mn.	S.	P.	
26·50	73·0	0·18	0·30	0·01	0·01	per cent.

In Curve 17 is given the relation of induction and force at the ordi-

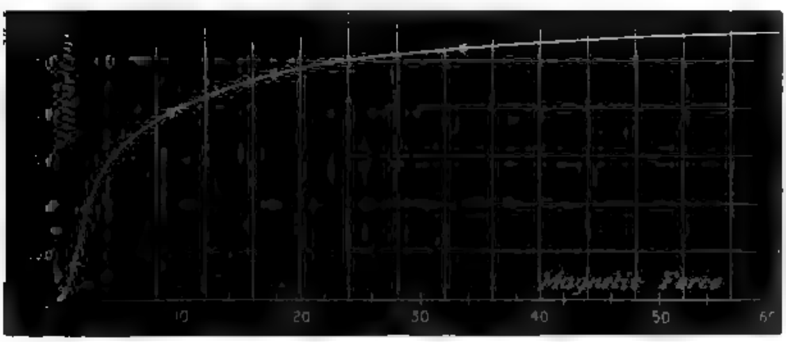
CURVE 16.

Magnetising Force, 30·3.



CURVE 17.

73 per cent. Nickel.



nary temperature. It is curious to remark that the induction for considerable forces is greater than in the steel with 33 per cent. of nickel, and that it is greater than for a mechanical mixture of iron and nickel in the proportions of the analysis, however the particles might be arranged in relation to each other.

The critical temperature of the material is 600° C.; it shows no material difference between the critical temperatures for increasing and diminishing temperatures.

II. "Photographic Determination of the Time-relations of the Changes which take place in Muscle during the Period of so-called 'Latent Stimulation.'" By J. BURDON SANDERSON, F.R.S. Received April 17, 1890.

It is now forty years since Helmholtz published his fundamental experiments on the time-relations of muscular contractions. The purpose of this investigation was to ascertain "the periods and stages in which the energy of muscle rises and sinks after instantaneous stimulation;" the word energy being defined as the "mechanical expression of activity;" and one of the most important conclusions of the author was that, in the muscles investigated by him, contraction does not begin until nearly one hundredth of a second after excitation. This interval has, by subsequent writers, been called the period of "latent stimulation."

Helmholtz subsequently (1854) showed, by experiments of surpassing ingenuity, that during this period an electrical change of very short duration occurs, which culminates at about one two-hundredth of a second after excitation. The fact discovered by Helmholtz was further investigated by Bernstein in 1866, with the aid of the repeating rheotome, and subsequently (1875) by du Bois-Reymond, whose statement of the actual time-relations of the electrical response to an instantaneous excitation of the gastrocnemius of the frog is embodied in a curve which denotes that the muscular surface becomes negative to the tendon about three thousandths of a second after excitation, that this effect culminates at seven thousandths of a second, and that it is immediately followed by a change of opposite sign, which culminates at about ten thousandths.

The statement enunciated above may be taken to represent the present state of knowledge on the subject of the "negative variation" or electrical response of muscle to an instantaneous stimulus; but, as regards the mechanical response, a great effort has been made of late years to obtain a more accurate measurement of the period of latent stimulation by methods founded on those originally employed by Helmholtz, with the result that it has been shortened very considerably. Two observers, viz., Professor Tigerstedt, of Stockholm, and more recently Professor Yeo, F.R.S., have, by improved methods, obtained records from which they conclude that the duration of the period is 0·005″. Finally, Professor Regecsky, of Pesth, has, by avoiding certain sources of error, obtained curves which lead him to conclude that the mechanical response may begin "at the moment of direct excitation"—in other words, that the period of latent stimulation does not exist.

I have now to submit to you incontrovertible evidence which the photographic method affords, not only that the estimate of the duration of the period of latent stimulation accepted as true ever since Helmholtz's early investigations is very much too long; but that the final conclusion arrived at by Dr. Yeo a year ago, that it has a real duration of five thousandths of a second, is erroneous. I am further in a position to demonstrate what are the time-relations of the electrical change to the muscular contraction with which it is associated.

The method of observation consists in projecting the movement to be recorded, whether of the muscle or that of any instrument which serves as an index of change, on a vertical slit on which the vibrations of a tuning-fork and the motion of a signal are also shadowed. Immediately behind the slit is a photographic plate, which is carried by an equilibrated pendulum. The approximately uniform rate of motion of the sensitive surface which receives the light-written record is about one meter per second, but is determined in each experiment by reference to the rate of vibration of a tuning-fork.

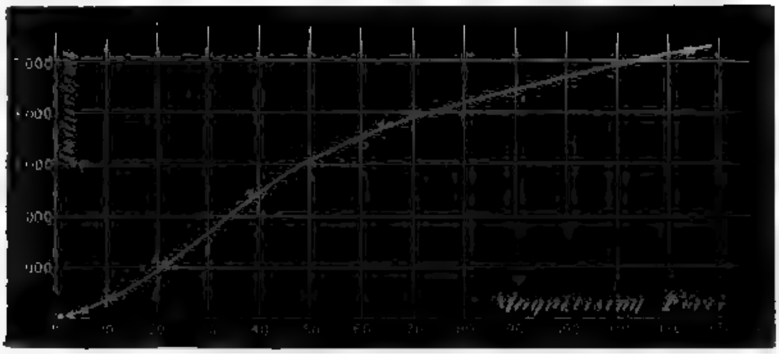
The plan adopted for obtaining a photographic record of the earliest trace of change of form, was based on the by no means new consideration that the effect of an instantaneous stimulus is in the first instance limited to the part of the structure to which it is applied, and, consequently, may fail to produce any measurable change of form of the whole muscle; the parts which first contract doing so at the expense of the as yet relaxed parts which are connected with them. This consideration is applicable not only to the case in which the muscle is excited directly, but also to that in which it is excited through its nerve; for in the latter case, each fibre is first stimulated at the spot at which it receives its nerve.

In the experiments on direct excitation, the muscles used were the *gastrocnemius* and *sartorius* of the frog. In the former the movement of contraction was communicated to a light index, which was supported by a fine spring. One end of the index rested on the muscle, while the other occupied the front focus of a projection apparatus, the slit being in the other focus. When the sartorius was used the surface of the muscle was itself brought for a moment into the focus, at the seat of excitation. The unavoidable exposure of the structure to the electric light, which this method involved, lasted scarcely more than a second. In successful experiments, the interval between excitation and the beginning of the contraction was $2\frac{1}{2}$ thousandths ($=\frac{1}{400}$) of a second.

In a photographic record of a succession of events no time-error is possible, provided that the rate of movement of the recording surface remains unaltered, for, if I may so express myself, an event cannot be seen photographically before it happens. It is therefore certain

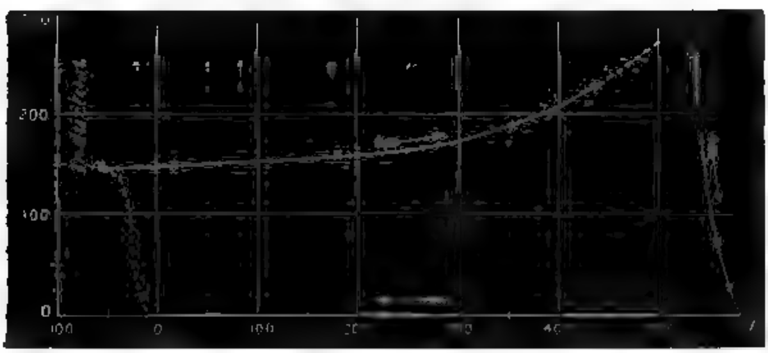
Curve 8.

25 per cent. Nickel.



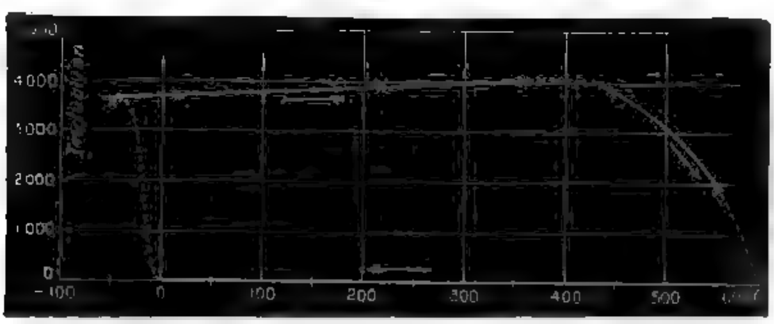
Curve 9.

Magnetising Force, 6·7. 25 per cent. Nickel.



Curve 10.

Magnetising Force, 64.



ing force 6·7; whilst Curve 10 shows the induction in terms of the temperature to a different scale for a force of 64. These curves show that, through a range of temperature from somewhat below freezing to 580° C., this material exists in two states, either being quite stable, the one being non-magnetisable, the other magnetisable. It changes from non-magnetisable to magnetisable if the temperature be reduced a little below freezing; the magnetisable state of the material does not change from magnetisable to non-magnetisable until the temperature is raised to 580° C.

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Curve 11.

observations of resistance, however, are indicated by the crosses in the neighbourhood of the letter A on the curve. The wire was then allowed to return to the temperature of the room, and was subsequently heated, the actual observations being shown by crosses on the lower branches of the curve, the heating was continued to a temperature of 680° C., and the metal was then allowed to cool, the actual observations being still shown by crosses. From this curve it will be seen that in the two states of the metal (magnetisable and non-magnetisable) the resistances at ordinary temperatures are quite different. The specific resistance in the magnetisable condition is about 0·000052; in the non-magnetisable condition it is about 0·000072. The curve of resistance in terms of the temperature of the material in the magnetisable condition has a close resemblance to that of soft iron, excepting that the coefficient of variation is much smaller, as, indeed, one would expect in the case of an alloy; at 20° C. the coefficient is about 0·00132; just below 600° C. it is about 0·0040, and above 600° C. it has fallen to a value less than that which it had at 20° C. The change in electrical resistance effected by cooling is almost as remarkable as the change in the magnetic properties.

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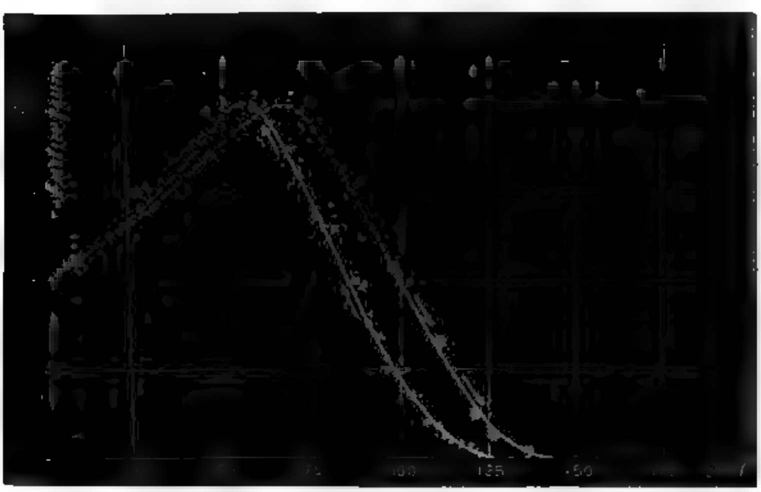
F. This sample contains 30 per cent. of nickel. Curve 12 shows the relation of induction to magnetising force at the ordinary temperature, and Curve 13 the relation of induction and temperature for

CURVE 12.

30 per cent. Nickel.

CURVE 13.



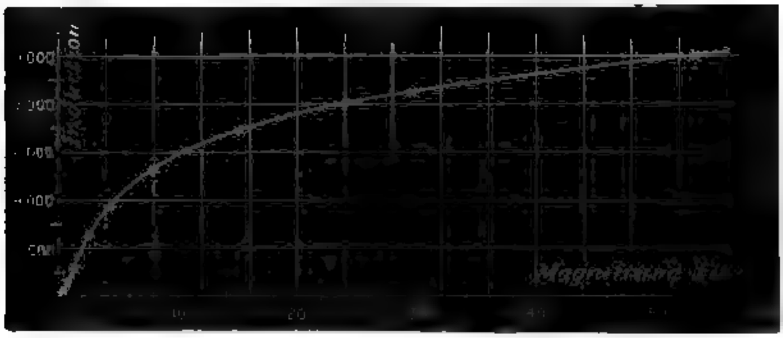
a force of 0·65. The remarkable feature here is the low temperature at which the change between magnetisable and non-magnetisable occurs, whether the temperature is rising or falling. Comparing it with the last sample, we see that the character of the material with regard to magnetism is entirely changed.

G. The analysis of this sample is—

Fe.	Ni.	C.	Mn.	S.	P.	
66·19	33·0	0·28	0·50	0·01	0·02	per cent.

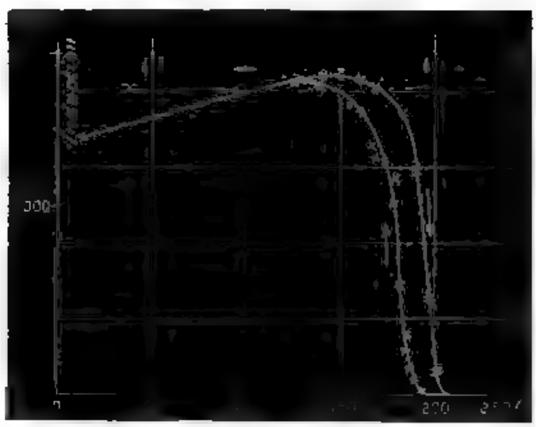
CURVE 14.

33 per cent. Nickel.



CURVE 15.

Magnetising Force, 1·0.



In Curve 14 is given the relation of induction and force at the ordinary temperature, and in Curves 15 and 16 the relation of induction and temperature for forces 1·0 and 30·3. The remarkable feature of this material is the complete difference from the last but one, and the low temperature of change. There is but very little difference between the temperatures of change when heated and when cooled.

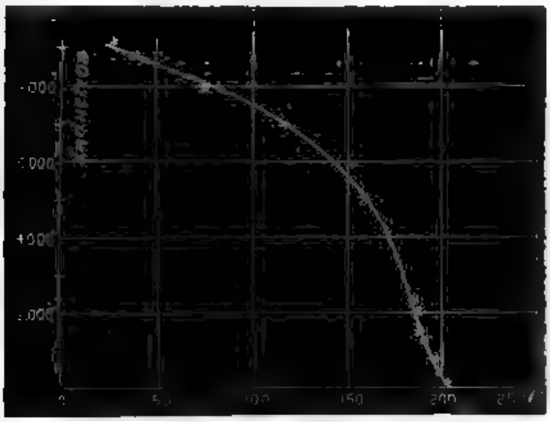
H. The analysis of this sample, as furnished by Mr. Riley, is—

Fe.	Ni.	C.	Mn.	S.	P.	
26·50	73·0	0·18	0·30	0·01	0·01	per cent.

In Curve 17 is given the relation of induction and force at the ordi-

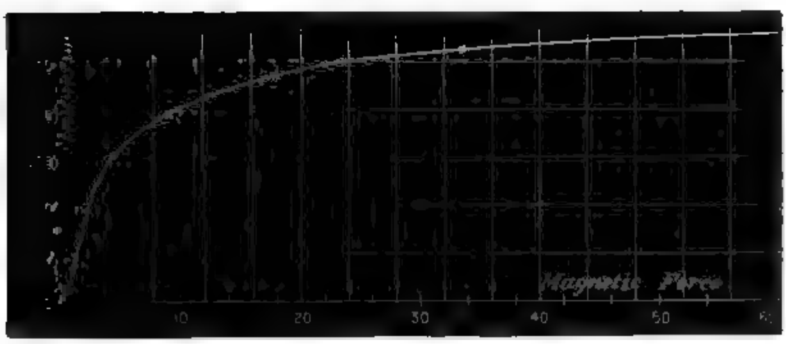
CURVE 16.

Magnetising Force, 30·3.



CURVE 17.

73 per cent. Nickel.



nary temperature. It is curious to remark that the induction for considerable forces is greater than in the steel with 33 per cent. of nickel, and that it is greater than for a mechanical mixture of iron and nickel in the proportions of the analysis, however the particles might be arranged in relation to each other.

The critical temperature of the material is 600° C.; it shows no material difference between the critical temperatures for increasing and diminishing temperatures.

II. "Photographic Determination of the Time-relations of the Changes which take place in Muscle during the Period of so-called 'Latent Stimulation.'" By J. BURDON SANDERSON, F.R.S. Received April 17, 1890.

It is now forty years since Helmholtz published his fundamental experiments on the time-relations of muscular contractions. The purpose of this investigation was to ascertain "the periods and stages in which the energy of muscle rises and sinks after instantaneous stimulation;" the word energy being defined as the "mechanical expression of activity;" and one of the most important conclusions of the author was that, in the muscles investigated by him, contraction does not begin until nearly one hundredth of a second after excitation. This interval has, by subsequent writers, been called the period of "latent stimulation."

Helmholtz subsequently (1854) showed, by experiments of surpassing ingenuity, that during this period an electrical change of very short duration occurs, which culminates at about one two-hundredth of a second after excitation. The fact discovered by Helmholtz was further investigated by Bernstein in 1866, with the aid of the repeating rheotome, and subsequently (1875) by du Bois-Reymond, whose statement of the actual time-relations of the electrical response to an instantaneous excitation of the gastrocnemius of the frog is embodied in a curve which denotes that the muscular surface becomes negative to the tendon about three thousandths of a second after excitation, that this effect culminates at seven thousandths of a second, and that it is immediately followed by a change of opposite sign, which culminates at about ten thousandths.

The statement enunciated above may be taken to represent the present state of knowledge on the subject of the "negative variation" or electrical response of muscle to an instantaneous stimulus; but, as regards the mechanical response, a great effort has been made of late years to obtain a more accurate measurement of the period of latent stimulation by methods founded on those originally employed by Helmholtz, with the result that it has been shortened very considerably. Two observers, viz., Professor Tigerstedt, of Stockholm, and more recently Professor Yeo, F.R.S., have, by improved methods, obtained records from which they conclude that the duration of the period is 0·005″. Finally, Professor Regecsky, of Pesth, has, by avoiding certain sources of error, obtained curves which lead him to conclude that the mechanical response may begin "at the moment of direct excitation"—in other words, that the period of latent stimulation does not exist.

I have now to submit to you incontrovertible evidence which the photographic method affords, not only that the estimate of the duration of the period of latent stimulation accepted as true ever since Helmholtz's early investigations is very much too long; but that the final conclusion arrived at by Dr. Yeo a year ago, that it has a real duration of five thousandths of a second, is erroneous. I am further in a position to demonstrate what are the time-relations of the electrical change to the muscular contraction with which it is associated.

The method of observation consists in projecting the movement to be recorded, whether of the muscle or that of any instrument which serves as an index of change, on a vertical slit on which the vibrations of a tuning-fork and the motion of a signal are also shadowed. Immediately behind the slit is a photographic plate, which is carried by an equilibrated pendulum. The approximately uniform rate of motion of the sensitive surface which receives the light-written record is about one meter per second, but is determined in each experiment by reference to the rate of vibration of a tuning-fork.

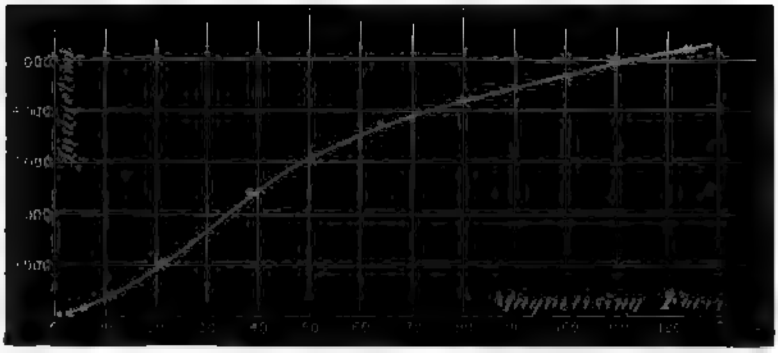
The plan adopted for obtaining a photographic record of the earliest trace of change of form, was based on the by no means new consideration that the effect of an instantaneous stimulus is in the first instance limited to the part of the structure to which it is applied, and, consequently, may fail to produce any measurable change of form of the whole muscle; the parts which first contract doing so at the expense of the as yet relaxed parts which are connected with them. This consideration is applicable not only to the case in which the muscle is excited directly, but also to that in which it is excited through its nerve; for in the latter case, each fibre is first stimulated at the spot at which it receives its nerve.

In the experiments on direct excitation, the muscles used were the *gastrocnemius* and *sartorius* of the frog. In the former the movement of contraction was communicated to a light index, which was supported by a fine spring. One end of the index rested on the muscle, while the other occupied the front focus of a projection apparatus, the slit being in the other focus. When the sartorius was used the surface of the muscle was itself brought for a moment into the focus, at the seat of excitation. The unavoidable exposure of the structure to the electric light, which this method involved, lasted scarcely more than a second. In successful experiments, the interval between excitation and the beginning of the contraction was $2\frac{1}{2}$ thousandths ($= \frac{1}{400}$) of a second.

In a photographic record of a succession of events no time-error is possible, provided that the rate of movement of the recording surface remains unaltered, for, if I may so express myself, an event cannot be seen photographically before it happens. It is therefore certain

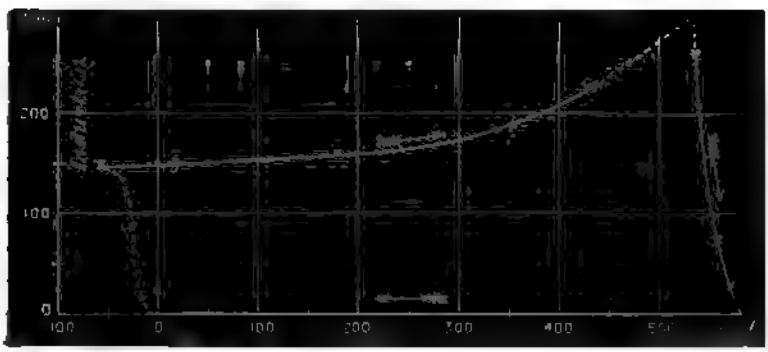
CURVE 8.

25 per cent. Nickel.



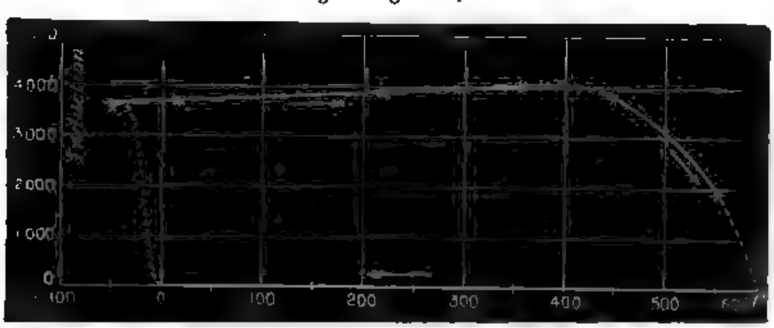
CURVE 9.

Magnetising Force, 6·7. 25 per cent. Nickel.



CURVE 10.

Magnetising Force, 64.



ing force 6·7; whilst Curve 10 shows the induction in terms of the temperature to a different scale for a force of 64. These curves show that, through a range of temperature from somewhat below freezing to 580° C., this material exists in two states, either being quite stable, the one being non-magnetisable, the other magnetisable. It changes from non-magnetisable to magnetisable if the temperature be reduced a little below freezing; the magnetisable state of the material does not change from magnetisable to non-magnetisable until the temperature is raised to 580° C.

The same kind of thing can be seen in a much less degree with ordinary steel. Over a small range this can exist in two states; but in changing its state from non-magnetisable to magnetisable a considerable amount of heat is liberated, which causes rise of temperature in the steel. It is observed in samples B and C of nickel steel, as we have just seen, but at a higher temperature.

As might be expected, the other physical properties of this material change with its magnetic properties. Mr. Riley has kindly supplied me with wire.

The wire as sent to me was magnetisable as tested by means of a magnet in the ordinary way. On heating it to a dull redness it became non-magnetisable, whether it was cooled slowly or exceedingly rapidly, by plunging it into cold water. A quantity of the wire was brought into the non-magnetisable state by heating it and allowing it to cool. The electric resistance of a portion of this wire, about 5 metres in length, was ascertained in terms of the temperature; it was first of all tried at the ordinary temperature, and then at temperatures up to 340° C. The specific resistances at these temperatures are indicated in Curve 11 by the numbers 1, 2, 3. The wire was then cooled by means of solid carbonic acid. The supposed course of change of resistance is indicated by the dotted line on the curve; the actual

CURVE 11.

in the same direction, and that at the cessation of the current the meniscus does not return, or returns very slowly, describing on the photographic plate a curve, of which the characters will be discussed in a paper to be shortly submitted to the Society by Mr. Burch. It is sufficient to say that the reason why the meniscus returns so slowly is that the potential of the charge which it has received is proportional to the displacement; in the case of a current of very short duration it is inconsiderable as compared with the difference of potential of the terminals at the moment that the current is broken.

When, instead of a single current in one direction (one two-hundredth second), two currents of the same duration follow each other in opposite direction, the record resembles in its essential characters that of the excitatory electrical response in muscle. It is seen that the mercurial column, which is displaced during the brief duration of the first current, returns abruptly to the previous position during the second. These phenomena I leave also to be discussed subsequently, noting only that the difference of potential at the terminals of the electrometer which is required to bring back the column to its original position is the same as that by which it was displaced, and that such an effect as has been described in muscle could not be produced by the becoming negative of the middle of each muscular fibre, unless that change were followed either by another in the opposite direction at the seat of excitation, or by a similar change at the other electrode.

Considering that the known velocity of propagation of the excitatory process in the muscle of the frog is about 3 meters, and that the distance between the contacts is about 1·5 cm., we should expect that if the two currents through the electrometer, the existence of which the photographic record so distinctly indicates, were due to propagation, they should follow each other at an interval of one two-hundredth of a second. The actual difference of time between the two electrical effects lies fairly within this estimate.

Postscript, April 28th.—Since sending this communication, I have become aware of a research published very recently by Professor Bernstein ("Ueber den mit einer Muskelzuckung verbundenen Schall und das Verhältniss desselben zur negativen Schwankung."—'Untersuch. aus dem Physiol. Institut der Universität Halle,' 1890), which relates closely to the subject of this paper. The facts observed, though of a different order from those recorded above, afford a remarkable confirmation of them. They are as follows:—When a muscle (gastrocnemius of a rabbit) is excited by a single induction current applied to its nerves, its tendon and muscular surfaces respectively being connected with a telephone, the electrical response can be heard telephonically. This Bernstein calls the electrical thud (*electrischer Stoss*). A thud of a similar character may be heard by

auscultation. If telephone and stethoscope are applied to the same ear, one sound only is heard. Exner has shown that any two sounds which are as much as $\frac{1}{500}$ of a second apart are audible as distinct sounds. Bernstein therefore concludes that, inasmuch as contraction begins nearly $\frac{1}{100}$ second after excitation, and the electrical change culminates at $\frac{1}{200}$ second, the mechanical thud must, as well as the electrical, be molecular, and concludes that the two sounds are coincident. In the second of these conclusions, Professor Bernstein appears to be justified, but not in the first. It having been shown by the photographic records that the two responses, the electrical and the mechanical, are nearly coincident, it is no longer needful to seek an explanation of the fact that the electrical and mechanical sounds are indistinguishable.

III. "The Development of the Sympathetic Nervous System in Mammals." By A. M. PATERSON, M.D. Communicated by A. MILNES MARSHALL, F.R.S. Received April 18, 1890.

(Abstract.)

The following investigations were undertaken with the object of determining the origin of the Mammalian sympathetic system, and of clearing up thereby certain points in its morphology.

Two opposite views exist at present among embryologists regarding its development. In both views the segmental formation of the sympathetic cord is upheld. According to the older view (Remak, &c.), it is mesodermal, and is formed *in situ*. According to more recent views, it is ectodermal. Balfour and Onodi, who have maintained the latter view, differ, however, as to the fundamental origin of the sympathetic system,—Balfour regarding each sympathetic ganglion as an offshoot from the spinal nerve, while Onodi considers it a direct proliferation from the spinal ganglion.

For the present research mammalian embryos were exclusively employed—rat, mouse, rabbit, and human embryos. The stage in development was first considered in which the sympathetic system was plainly visible, and from this point the earlier and later stages in the process were traced. It was only possible to determine approximately the ages of the embryos employed, as the time of impregnation varies in different instances, and two embryos from the same uterus often differ in size and extent of development.

The first event to occur is the formation of the main sympathetic cord. In very young embryos (*e.g.*, rabbit, 7 days, axial length 5 mm.), in which the spinal nerves are completely formed and the spinal ganglia clear and distinct, there is no trace of the sympathetic

ganglia or the connecting branches with the spinal nerves. The cord is first seen in transverse and sagittal sections of mouse and rat embryos of about 8 days. It arises on either side as a solid, uniform, unsegmented rod of fusiform cells, imbedded in the mesoblast surrounding the aorta, and lies in the interval between the latter and the adjacent veins. Slightly thicker anteriorly than posteriorly, it ends abruptly in front at the level of the first vertebral segment; behind it becomes indistinct posteriorly to the suprarenal body, to which it sends a considerable cellular bundle, and, tapering off, disappears at the level of the hind limbs. This cellular column is formed by the differentiation of the mesoblastic cells *in situ*; it is not connected with the spinal nerves, and it is unsegmented.

The next step consists in the junction of the spinal nerves on either side with these columns of cells. This is effected by the gradual growth of the inferior primary division of the nerve and its final division at the junction of the body wall (somato-pleure) and splanchnic area (splanchno-pleure) into *somatic* and *splanchnic* branches (rat, mouse, 8–9 days). The former passes on to be distributed in the body wall. The latter can be followed in succeeding stages in a ventral and mesial direction, until at last it meets and joins the cellular sympathetic cord (mouse, 11 days, rat, 12 days).

The origin of this splanchnic branch is from both roots of the spinal nerve, of which the ventral root contributes the greater number of fibres. At its peripheral end it terminates in one of two ways. At the anterior part of the thorax the fibres seem to end entirely in the sympathetic cord; that is, they have not been traced beyond it. In the posterior thoracic and in the lumbar regions the splanchnic branch, on reaching the sympathetic cord, divides into two parts, of which one joins the cord, the other passes over it. In both cases the fibres which join the cord are directly connected with the component cells.

In certain regions no such connexions can be made out. Behind the kidney and the bifurcation of the aorta (*i.e.*, behind the loins) the splanchnic branches cease. In front of the fore limbs (*i.e.*, in the neck) the splanchnic branches do not join the sympathetic system. In comparatively advanced embryos, distinct nerves, morphologically similar to the splanchnics, course inwards round the vertebral artery to the tissues surrounding the growing vertebræ, but at the same time occupying a position dorsal to the sympathetic cord, and altogether unconnected with it.

These splanchnic branches correspond to the white *rami communicantes*.

The formation of the ganglia on the main sympathetic cord occurs subsequently, and is subordinate to the connexions with it of the splanchnic branches of the spinal nerves. Up to the time of the for-

mation of cartilaginous vertebral *centra*, there is no constriction of the main cord (mouse, 17—18 days). Gangliation begins at and after this date, and is due, in the first place, and principally, to the junction of the splanchnic branches; this causes the accession of a large number of nerve-fibres at the point of entrance, and the consequent persistence of the component cells (which are joined by these nerves), as ganglion cells. Gangliation is caused, secondly, and to a less extent, by the anatomical relations of the sympathetic cord to the bony segments, vessels, &c., which are developed near it, and which, by their growth, cause indentation or constriction of the cord at certain points.

This view is supported by the evidence obtained from the dissection of human embryos of different ages (3rd, 4th, 5th, and 6th months), where the cord has the form of a band or strip, constricted irregularly at considerable intervals, rather than of a regularly nodulated chain; and by the evidence derived from the normal adult structure, where the "segmentation" of the sympathetic cord is apparent rather than real.

The cervical portion of the embryonic sympathetic cord is at first undifferentiated from the main column. Growing with the growth of the neck, it separates at the origin of the vertebral artery, into two unequal parts. The smaller part forms a fibro-cellular cord, which accompanies that artery, and forms the vertebral plexus. The other, or main, portion accompanies the carotid vessels. Growing rapidly, it becomes constricted off from the main sympathetic cord by the formation of a gradually elongating fibro-cellular commissure, and gives rise to the "superior cervical ganglion." This lies alongside the internal carotid artery, and gives off anteriorly a fibro-cellular bundle, which accompanies and is finally lost upon that vessel, as the carotid plexus. When the middle cervical ganglion is present, it may be looked upon as representing a mass of the original cells of the sympathetic cord, which have been included in the growth of the commissure connecting the main cord to the superior ganglion. These parts may be regarded as belonging to the collateral distribution of the sympathetic system, because (1) they are outgrowths from the main cord, and (2) they receive no splanchnic branches directly from the spinal nerves.

The caudal termination of the sympathetic system is likewise an outgrowth from the main cord. In the youngest embryos in which it is found (rat, 8 days), the cord is lost at the level of the hind limbs; at a later period of development (rat, 12 days) it reaches further, to the bifurcation of the aorta; while in still older embryos it can be traced alongside the middle sacral artery for a considerable distance. It is not joined by splanchnic branches, and it is only in an advanced stage of development (rat, 22 days) that trans-

verse cellular communications take place on the dorsal aspect of the middle sacral artery between the cords of opposite sides to produce the *ganglion impar* and the loop of connexion between the caudal ganglia.

The peripheral branches from the sympathetic cord arise as cellular buds or outgrowths which are first seen about the time when the splanchnic branches of the spinal nerves join the cord (mouse, rat, 11—12 days; human embryo 1st month). They accompany the parts of the splanchnic branches which do not join the sympathetic cord into the splanchnic area; and, especially in the hinder thoracic region, form considerable masses traceable along the main vessels, which in older embryos give rise to parts of the splanchnic nerves, as well as the medullary portions of the suprarenal bodies, as previous observers have described.

The gray *rami communicantes* may (doubtfully) be said to belong to the category of peripheral branches from the sympathetic cord. They appear to arise from the cord as cellular outgrowths which pursue a centripetal course along the splanchnic branches of the spinal nerves towards their roots; but in regions where these are absent, or are unconnected with the sympathetic cord, I have not been able to satisfy myself about their formation.

The principal conclusions derived from these investigations are that the sympathetic system in Mammals is mesoblastic, is formed *in situ* out of the cellular tissue surrounding the embryonic aorta, and is at first entirely independent of the cerebro-spinal nervous system; it is primarily uniform and unsegmented, in this respect resembling the organs in the splanchnic area—the vascular and alimentary systems—with which it is so closely related, functionally as well as structurally. It becomes secondarily connected with certain spinal nerves by the growth from the latter of the white *rami communicantes*, and in consequence becomes gangliated in an irregular manner. From the main cord cellular outgrowths arise which form peripheral, non-medullated nerves, plexuses, and ganglia, as well as the medullary portions of the suprarenal bodies.

Morphologically, the Mammalian sympathetic cord resembles the structures with which it is in structural and functional relation, in being mesoblastic, and in its development primarily unsegmented. It is a rod, fibro-cellular in structure, out of which, on the one hand, are produced certain ganglia and nerves, and which, on the other hand, becomes connected with certain cerebro-spinal fibres—the splanchnic branches of the spinal nerves. The mechanism thus produced may be regarded as providing for the guidance of these fibres (*e.g.*, vasomotor nerves) to their destinations, and as regulating their proper distribution to the vascular and alimentary systems.

In conclusion, I wish to express my indebtedness to Professor

Milnes Marshall, of Manchester, for his advice and criticism, and for many valuable suggestions made after reading the memoir of which this is a summary.

IV. "A Note on an Experimental Investigation into the Pathology of Cancer." By CHARLES A. BALLANCE and SAMUEL G. SHATTOCK. Communicated by Sir JAMES PAGET, Bart., F.R.S. Received April 15, 1890.

[Publication deferred.]

Presents, May 1, 1890.

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The Author.

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May 8, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On certain Ternary Alloys. Part II." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.C.S., F.I.C., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received April 3, 1890.

In Part I, it has been shown that when the three metals lead, zinc, and tin are fused together and well intermixed, and the mixture allowed to stand molten for some hours at a nearly constant temperature, a single homogeneous alloy results if the proportion of tin present exceeds a certain limiting amount (about three-eighths of the entire mass); but with smaller proportions of tin the mass divides itself into two different ternary alloys of unequal density: the heavier contains chiefly lead, together with some of the tin, and as much zinc as the lead can dissolve in presence of the particular proportion of tin associated with it; whilst the lighter mainly consists of zinc, with the rest of the tin, and as much lead as the zinc can dissolve in presence of that tin.

We have found that analogous results are obtained with various other ternary mixtures of metals, A, B, C, such that whilst A and B are not miscible together in all proportions (like lead and zinc), C is miscible in all proportions with either A or B separately. Of such mixtures, the following are examples;—

Heavier metal, A.	Lighter metal, B.	Third metal, C.
Lead.	Zinc.	Tin.
Lead.	Zinc.	Silver.
Lead.	Zinc.	Cadmium.
Lead.	Zinc.	Antimony.
Bismuth.	Zinc.	Tin.
Bismuth.	Zinc.	Silver.
Lead.	Aluminium.	Tin.
Lead.	Aluminium.	Silver.
Bismuth.	Aluminium.	Tin.
Bismuth.	Aluminium.	Silver.

We have made a large number of experiments with various mixtures of this kind. So far as our results are completed, they lead to the following general conclusions :—

1. In all cases, the mixture of the three metals A, B, C, when allowed to stand molten for a sufficient length of time at a tolerably equable temperature, divides itself into two different ternary alloys of unequal density, if the proportion of C present in the entire mass falls below a certain limiting amount; but, if the quantity of C present is above this limit, no such separation takes place, only one homogeneous alloy resulting.

2. Under ordinary circumstances, the different alloys thus formed are respectively a saturated solution of A in a mixture of B and C (lighter alloy), and one of B in a mixture of A and C (heavier alloy); the solubilities being such that, the greater the proportion of C present, the more of A (or B) is dissolved. Certain metals, however, appear to be capable of forming true chemical compounds in atomic proportion, in which case the quantity of A (or B) dissolved does not always vary directly with the amount of C present.

3. The quantity of B dissolved by a given weight of A (or of A dissolved by a given weight of B) in presence of a given weight of C varies considerably with the nature of C. Moreover, although in certain cases (*e.g.*, the lead-zinc-tin alloys examined in Part I) a considerable variation in temperature makes hardly any measurable difference in the solubility, this is very far from being the general rule; the ordinary effect of increment in temperature is to increase the solubility of A in BC, and of B in AC, in some cases to a very considerable extent.

4. The third metal C divides itself between the two alloys in a fashion variable not only with the nature of A, B, and C, and with the temperature, but also with the relative proportions subsisting between A and B in the entire mass, and with the proportions of C contained therein. If curves be drawn, as described in Part I, with the percentages of C in one alloy as abscissæ and the differences in percentage between the two alloys as ordinates, two classes of curves may be distinguished. In one, the percentage in the lighter alloy is greater than that in the heavier; calling the difference +, the curve *rises* from the origin above the base line. In the other, the percentage in the lighter alloy is less than that in the heavier one; so that the difference is now —, and the curve *falls* from the origin below the base line. With curves of the first kind, it generally happens that the ordinate value increases gradually to a maximum and then diminishes; with some metals (*e.g.*, silver-lead-zinc) the diminution is only just perceptible; with others (*e.g.*, silver-bismuth-zinc) it is more marked; whilst with some (*e.g.*, tin-lead-zinc) it is carried so far that at length the ordinate becomes 0, and subsequently — in

sign, *i.e.*, the curve first rises above the base line to a maximum, and then sinks again, and crosses the base line, falling below it. Similarly, with curves of the second kind, it sometimes happens that the ordinate value reaches a negative maximum, and then lessens again, so that the curve again approaches the base line; as yet, however, we have not met with a case where the curve actually crosses the line giving a + ordinate value.

Mixtures of Lead, Zinc, and Tin at Higher Temperatures.

The experiments described in Part I indicated that little if any difference is produced in the composition of the two alloys into which a given mass of these three metals separates by varying the temperature at which the fused mass is maintained between 565° and 689°, or between about 650° and 750°; sensibly the same solubility curves for zinc in lead-tin, and for lead in zinc-tin, resulting in all cases. We find, however, that if a higher temperature be employed, 750—850° and upwards, a measurable increment in solubility is produced. At this more elevated temperature, the volatility of zinc is considerably enhanced, so that the ratio between the lead and zinc present in the compound ingot finally obtained differs more from that in the mass of metals originally weighed up than was the case in the experiments described in Part I; in those experiments, the average loss by oxidation and volatilisation per 100 parts of original metals jointly was nearly 4 parts, of which scarcely anything was due to loss of tin, about one-third to loss of lead, and some two-thirds to loss of zinc. In the experiments described below, 100 parts of original metals lost on an average about 10 or 12 parts altogether, almost the entire increment in the loss being due to enhanced volatilisation of zinc, this larger amount being due not only to the higher temperature, but also to the longer time of fusion (some twenty-four hours instead of eight). To reduce this loss by volatilisation to a minimum, the molten metals, when poured into the red-hot narrow clay test-tubes, were covered with layers of fused cyanide of potassium some 12 or 15 millimetres thick.

The melting arrangements employed throughout in the experiments described in this paper were substantially those detailed in Part I: *viz.*, the weighed metals were fused with cyanide of potassium in a clean crucible, well stirred together for some time, and poured into a red-hot clay test-tube, which was then kept hot for some hours by immersion in a bath of molten lead, fused in an iron cylindrical vessel surrounded by a clay jacket, several large Bunsen flames playing into the interspace being the source of heat. The temperature was ascertained from time to time by means of the platinum specific heat pyrometer, as described in Part I. It was

found more convenient to surround the clay test-tubes with thin wrought iron coverings made of wider tubes closed at the end; by so doing, the chance of spoiling an experiment by the infiltration of lead from the bath through minute cracks in the clay was avoided; moreover, by simply removing from the iron tubes the inner clay test-tubes by means of tongs, a new set of test-tubes containing fused mixtures could be readily introduced into the lead-bath without extinguishing the heating flames; whereas, when the clay test-tubes were plunged directly into the molten lead, the layer of fritted litharge that formed on the outer surface of the lead-bath sometimes rendered it difficult to remove the clay tubes without agitation.

Somewhat smaller ingots than those previously used were mostly prepared, usually weighing 50—60 grams, instead of 80 and upwards; the compound ingots ultimately formed were generally about 7 or 8 centimetres long, and 10—12 millimetres diameter.

The following solubility values for pure lead in pure zinc, and *vice versâ* (no tin being present), were obtained in a number of different observations.

Percentage of Zinc in Heavier Alloy.

At 565—750° (Part I).	At 750—850°.
1·14	1·23
1·22	1·27
1·30	1·28
1·30	1·34
	1·35
	1·36
Mean.... 1·24	Mean.... 1·30

Percentage of Lead in Lighter Alloy.

At 565—750° (Part I).	At 750—850°.
1·08	1·40
1·10	1·57
1·17	1·64
1·22	1·67
Mean.... 1·14	Mean.... 1·57

It hence results that the solubility of zinc in lead is greater at a temperature near to an average of 800° than at one near to an average of 650° by an amount only just perceptible, and barely outside the limits of experimental error; whilst the solubility of lead in zinc is increased nearly in the proportion of 3 to 2 by the same

temperature increment. The values described below show that when tin is also present the increment in solubility with higher temperature is in each case well marked.

The following mean values were obtained from twelve compound ingots, prepared by fusion for twenty-four hours at a temperature averaging near to 800° of mixtures containing originally equal weights of zinc and lead with varying proportions of tin :—

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
0	98·70	1·30	0	1·57	98·[illegible]	0
7·01	89·87	3·12	9·28	5·39	[illegible]·33	2·27
12·77	82·98	4·25	16·36	8·00	75·64	3·59
19·29	73·17	7·54	21·56	10·04	68·40	2·27
22·84	[illegible]	11·36	23·94	11·39	64·67	1·10
27·12	54·98	17·90	26·62	12·13	[illegible]·25	−0·50
29·47	50·07	[illegible]·46	28·37	12 95	58·68	−1·10
31·66	45·74	22·60	..	..	..	..

Fig. 1.

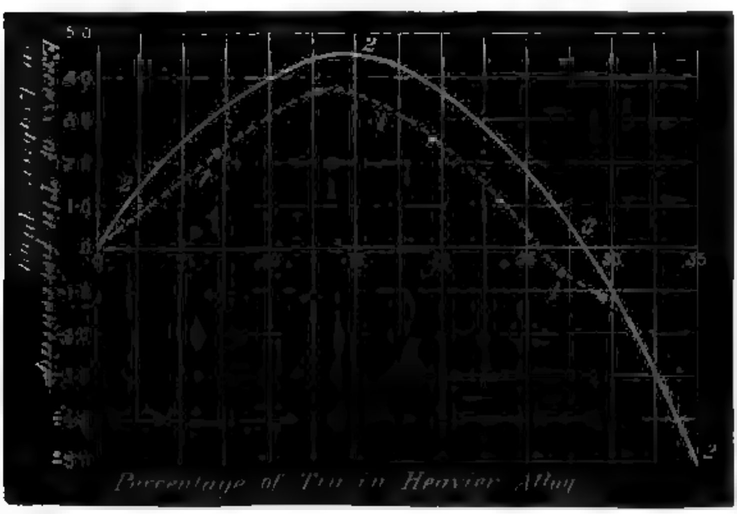


Fig. 1 represents the curve illustrating the tin distribution derived from these figures, the numbers in the first column being plotted as abscissæ and those in the last as ordinates (No. 1); the corresponding mean curve deduced from the observations at lower temperatures

(averaging near 650°), described in Part I, being represented by No. 2, in each case equal quantities of lead and zinc and varying proportions of tin being employed for the mixtures originally fused. The effect of the higher temperature is apparently to give a curve somewhat underlying that obtained at the lower temperature, and crossing the base line sooner; but the difference is not extremely great, and is probably at least partly due to the circumstance that the greater volatilisation of zinc at the higher temperature causes the proportion of lead relatively to zinc in the entire mass to rise higher: as shown in Part I, the tin distribution curve obtained with two parts of lead to one of zinc in the original mixtures underlies that obtained with equal proportions of the two metals, and crosses the base line sooner.

When the percentages of tin and zinc in the heavier alloys are plotted as abscissæ and ordinates respectively, a curve is obtained sensibly *overlying* that previously obtained at about 650°; and similarly with the curve obtained by plotting the percentages of tin and lead in the lighter alloys as abscissæ and ordinates respectively.

The following tables exhibit the solubility values deduced from the mean curves thus graphically obtained, the corresponding values for 650° being annexed for the sake of comparison:—*

Solubility of Zinc in Lead-Tin.

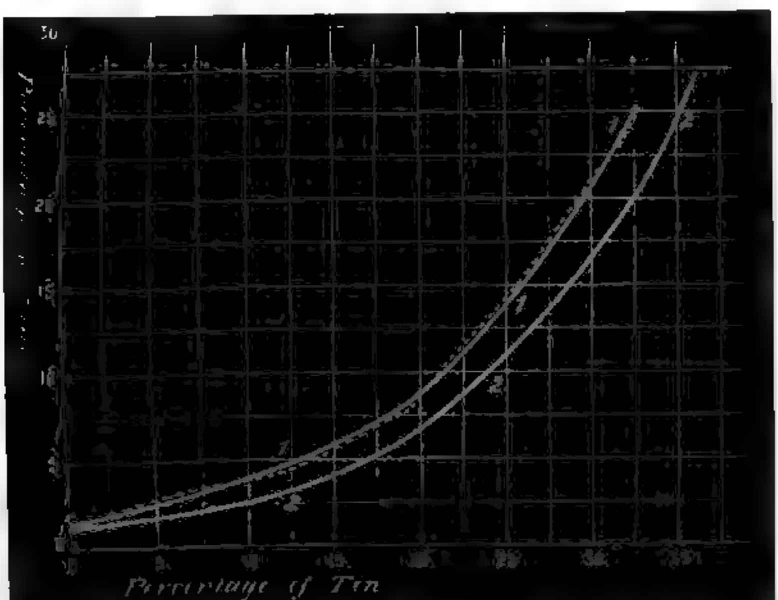
Per cent. of tin.	Temperature near 650°.		Temperature near 800°.	
	Per cent. of zinc.	Difference.	Per cent. of zinc.	Difference.
0	1·24		1·30	
2	1·44	0·20	1·80	0·50
4	1·65	0·21	2·30	0·50
6	1·89	0·24	2·80	0·50
8	2·15	0·26	3·30	0·50
10	2·45	0·30	3·80	0·50
12	2·85	0·40	4·40	0·60
14	3·4	0·55	5·10	0·70
16	4·1	0·7	5·9	0·80
18	5·0	0·9	6·9	1·00
20	6·1	1·1	8·1	1·2
22	7·5	1·4	9·6	1·5
24	9·25	1·75	11·6	2·0
26	11·4	2·15	14·1	2·5
28	13·9	2·5	17·1	3·0
30	16·7	2·8	20·4	3·3
32	19·8	3·1	24·0	3·6
34	23·2	3·4	..	..
36	27·0	3·8	..	..

* By inadvertence, some numerical inaccuracies exist in the table of solubility of

Solubility of Lead in Zinc-Tin.

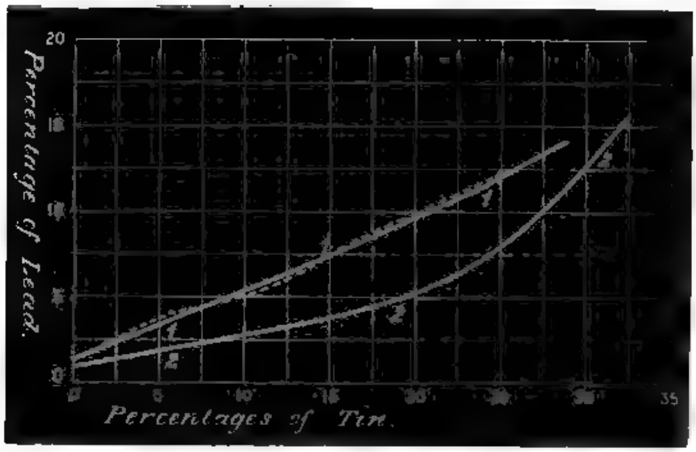
Per cent. of [illegible]	Temperature near 650°.		Temperature near 800°.	
	Per cent. of lead.	Difference.	Per cent. of lead.	Difference.
0	1·14		1·57	
2	1·47	0·33	2·35	0·78
4	1·80	0·33	3·15	0·80
6	[illegible]	0·33	3·95	0·80
8	2·46	0·33	4·75	0·80
10	[illegible]	0·34	5·55	0·80
12	3·14	0·34	6·35	0·80
[illegible]	3·50	0·36	7·15	0·80
16	3·9	0·40	7·95	[illegible]
18	4·5	[illegible]	8·75	0·80
20	5·3	[illegible]	9·55	[illegible]
22	6·3	1·0	10·35	0·80
24	7·5	1·2	11·20	0·85
26	8·9	1·4	[illegible]	0·85
28	10·6	1·7	12·90	0·85
30	12·75	2·15	..	..
32	15·5	2·75	..	..

FIG. 2.



zinc in lead-tin given in Part I for the abscissæ values (tin percentages) 26 and upwards; the corrected figures are given above.

Fig. 3.



Figs. 2 and 3 represent these values respectively, the curves marked 2 being those obtained at the lower temperatures (near 650°), and those marked 1 being the corresponding higher temperature mean curves (near 800°); the dotted lines being those connecting the points actually observed at near 800°.

Mixtures of Lead, Zinc, and Silver.

Owing to the lesser degree of fusibility exhibited by some of these mixtures, it was found necessary to employ throughout a temperature ranging between 750° and 850°, and generally pretty close to 800°. It is well known that on adding zinc to melted argentiferous lead the zinc rises to the top, carrying most of the silver present with it along with a little lead; whence, evidently, the curve representing the distribution of silver between the lighter and heavier alloys formed resembles that obtained with lead, zinc, and tin alloys when the proportion of tin present is but small, *i.e.*, the curve at first ascends above the base line; but no information appears to be extant giving any clue as to whether this kind of distribution would also be observed with mixtures containing large proportions of silver, or whether such mixtures would behave like lead-zinc-tin mixtures containing large proportions of tin, *i.e.*, furnishing a curve descending again to, and finally dropping below, the base line.

The analysis of the lead-zinc-silver alloys formed was made as follows:—a weighed quantity, usually some 5 or 6 grams, was dissolved in nitric acid and the solution diluted with so much hot water that on adding enough dilute hydrochloric acid to precipitate all the

silver present no lead chloride separated. The turbid liquid was kept hot in the water-bath for an hour or two, until the silver chloride had subsided, and was then filtered hot, the silver chloride being boiled up two or three times with water to wash out any lead chloride that might possibly have separated. The filtrate was evaporated with pure sulphuric acid in excess, and the lead sulphate formed separated and determined in the usual way. As with the lead-zinc-tin alloys described in Part I, it was found necessary to precipitate the zinc contained in the filtrate from the lead sulphate as sulphide, and to redissolve this (after filtration) in hydrochloric acid and precipitate whilst boiling as carbonate by sodium carbonate, finally weighing as ZnO; if the precipitation as sulphide were omitted, sensibly too high values were obtained from the presence of *lime*, presumably derived from the glass and porcelain vessels used. The ZnO, after weighing, was dissolved in hydrochloric acid and supersaturated with ammonia, and the trifling precipitate of alumina and ferric oxide (derived from the crucibles and clay test-tubes) estimated and subtracted. The figures given below are in all cases calculated upon the sum of the silver, lead, and zinc thus found

Series I.—Time of Fusion, 8 hours. Temp., 750—850°.

Percentage of silver in mixture before fusion.	Heavier alloy.			Lighter alloy.			Excess of silver percentage in lighter alloy over that in heavier.
	Silver.	Lead.	Zinc.	Silver.	Lead.	Zinc.	
0	0	98·70	1·30	0	1·57	98·43	0
3	0·02	98·25	1·73	5·75	1·95	92·30	5·73
6	0·12	98·36	1·52	12·32	5·12	82·56	12·20
9	0·34	97·16	2·50	17·23	9·00	73·77	16·89
10	0·18	97·67	2·15	18·96	8·62	72·42	18·78
12	0·19	97·39	2·42	20·95	11·81	67·24	20·76
14	0·37	97·81	1·82	24·10	9·06	66·84	23·73
16	0·60	96·55	2·85	28·85	7·34	63·81	28·25
18	0·85	97·23	1·92	33·77	3·04	63·19	32·92
20	1·19	95·46	3·35	34·86	2·83	62·31	33·67
22·5	1·38	96·40	2·22	38·33	2·78	58·93	36·95
25	1·61	96·00	2·39	39·95	2·86	57·19	38·34
27·5	1·82	95·73	2·45	41·90	3·27	54·83	40·08
31	2·52	95·10	2·38	46·43	3·66	49·91	43·91
36	3·01	95·59	1·40	53·74	3·70	42·56	50·73
38	3·58	95·23	1·19	54·15	3·53	42·32	50·57
41	5·30	93·47	1·23	56·67	4·32	39·01	51·37
44	8·31	90·12	1·57	60·13	7·06	32·81	51·82
47·5	11·45	86·70	1·85	62·94	9·54	27·52	51·49
52·5	14·06	84·10	1·84	65·48	10·50	24·02	51·42
57·5	17·14	80·43	2·43	66·19	12·85	20·96	49·05
62·5	19·45	77·62	2·93	66·24	14·78	18·98	46·79
65	..	..	..	67·39	16·66	15·95	..

Series II.—Time of Fusion, 24 hours. Temp., 750°—850°.

Percentage of silver in mixture before fusion.	Heavier alloy.			Lighter alloy.			Excess of silver percentage in lighter alloy over that in heavier.
	Silver.	Lead.	Zinc.	Silver.	Lead.	Zinc.	
0	0	98·70	1·30	0	1·57	98·43	0
5	0·02	98·64	1·34	9·34	2·51	88·15	9·32
7·5	0·10	98·38	1·52	15·46	4·63	79·91	15·36
10	0·32	97·61	2·07	22·07	6·96	70·97	21·75
12·5	0·25	97·08	2·67	22·28	7·56	70·16	22·03
15	0·88	96·23	2·89	27·95	6·17	65·88	27·07
20	1·12	96·98	1·90	39·49	3·46	57·05	38·37
25	1·46	96·58	1·96	41·82	3·90	54·28	40·36
27·5	1·60	97·14	1·26	48·11	3·46	48·43	46·51
30	2·26	96·46	1·28	48·93	3·82	47·25	46·67
35	5·35	93·27	1·38	51·86	4·47	43·67	46·51
40	7·78	90·76	1·46	53·98	4·78	41·24	46·20
45	9·36	88·62	2·02	61·07	7·23	31·70	51·71
50	10·75	86·95	2·30	61·93	10·28	27·79	51·18
60	..	..	..	68·89	18·28	12·83	..

(after correction) as 100. In the case of the lighter alloys, where zinc was the main constituent, as a rule the silver and lead only were determined, and the zinc taken by difference; but with the heavier alloys the zinc was invariably directly determined.

Series I is derived from the examination of thirty-eight compound ingots, and Series II from seventeen, several of the analyses quoted being the mean compositions derived from two mixtures nearly alike. The average loss by volatilisation and oxidation was about 4 grams out of 50—60 = about 8 per cent., in Series I, and 6 or 7 grams = about 13 per cent., in Series II, the zinc being the metal chiefly affected.

On plotting these figures as curves, it is noticeable, firstly, that the distribution of silver between the two alloys formed is such as throughout to yield a curve overlying the base line, and exhibiting a rise to a maximum, and subsequent slight fall. Thus curves 1 and 2, fig. 4, represent the numbers in Series I and II respectively, the percentages of silver in the *lighter* alloys being here taken as abscissæ, and the figures in the last column as ordinates.

Next, the curves obtained by plotting the percentages of silver and lead in the lighter alloys as abscissæ and ordinates respectively are most remarkable (curves 1 and 2, fig. 5). At first the two curves do not coincide, but they present the same general feature of rising to a first maximum, and then falling again to a point but little above the starting level, after which the two do not differ from one another

Fig. 4.

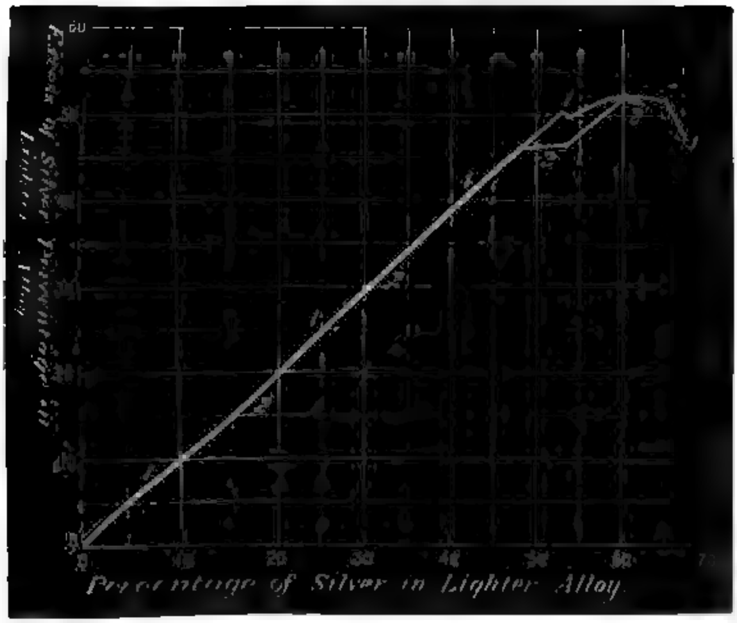
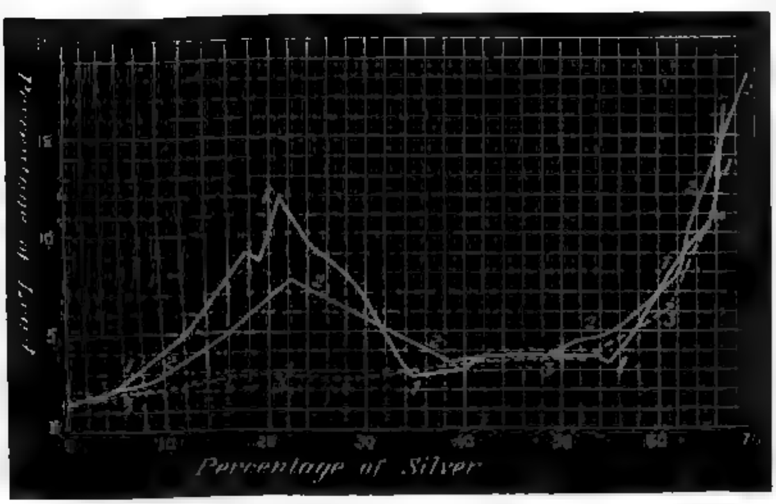

Excess of Silver percentage in Lighter Alloy
Percentage of Silver in Lighter Alloy

Fig. 5


Percentage of Lead
Percentage of Silver

by amounts outside the limits of variation ascribable to unavoidable differences of average temperature and fluctuations of temperature in the several experiments, both rising slowly until a point is reached when a very marked change takes place in the rate of ascent.

It is especially noticeable that the position of the first maximum is in each case close to that when the ratio between silver and zinc in the alloy is indicated by the formula $AgZn_5$—

	Silver.	Zinc.	Lead.	Ratio of zinc to silver.
Series I...............	20·95	67·24	11·81	1 to 0·313
Series II...............	22·28	70·16	7·56	1 „ 0·317
Calculated for $AgZn_5$				1 „ 0·332
„ $AgZn_4$				1 „ 0·277

Further, the points where the rate of ascent suddenly becomes much more rapid are in neither case far from that indicated by the formula Ag_4Zn_5—

	Silver.	Zinc.	Lead.	Ratio of zinc to silver.
Series I, between....	53·74	42·56	3·70	1 to 1·26
	56·67	39·01	4·32	1 „ 1·45
		Mean		1 „ 1·325
Series II, about	53·98	41·24	4·78	1 „ 1·31
Calculated for Ag_4Zn_5				1 „ 1·33
„ $AgZn$				1 „ 1·66
„ Ag_2Zn_3				1 „ 1·11

As regards these points, it is remarkable that alloys where upwards of nine-tenths of the whole consists of silver and zinc in the proportion Ag_4Zn_5 exhibit a distinct coppery red hue when a recently filed or polished surface is exposed to the air for a short time. As the proportion of Ag_4Zn_5 present diminishes owing to the presence of excess of silver, or of zinc, or of either with additional lead, the red colour becomes less marked, the tint being only a pale yellow, or even entirely white, when the alloys contain less silver and more zinc, or more silver and less zinc, than about the limits—

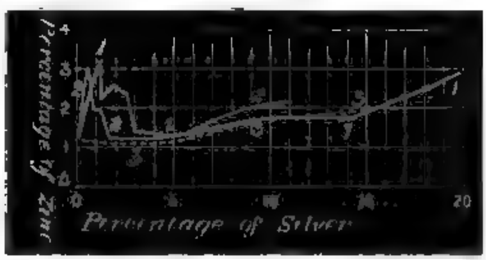
Silver	42 per cent.	......	62 per cent.
Lead	3 „	......	10 „
Zinc	55 „	......	28 „
	100		100

corresponding with a percentage of Ag_4Zn_5 of about 73 in the first case and 65 in the second.

It is further remarkable that mixtures of bismuth, zinc, and silver, on standing fused for some eight hours, separate into different ternary alloys, the lighter of which yield curves exhibiting exactly the same peculiarities as those above mentioned, viz., rise to maximum near $AgZn_5$, fall again to a minimum, and then a slower rise again to Ag_4Zn_5, marked by a red colour. These alloys will be discussed in a subsequent paper.

Thirdly, the curves obtained by plotting the percentages of silver in the heavier alloys as abscissæ and those of zinc as ordinates (Nos. 1 and 2, fig. 6) are in each case such as to indicate that, as long

FIG. 6.



as the mixture of metals used contains less than about 41 per cent. of silver in the first case and 28 in the second, the heavier alloy that separates invariably contains more zinc than that formed either when no silver at all is present, or when these limiting proportions are reached. At these limiting proportions the percentage of zinc present is slightly *lower* than that found in binary zinc alloy, as also is the proportion of zinc calculated per unit of lead, thus—

	Silver.	Zinc.	Lead.	Ratio of lead to zinc.
Binary alloy	0	1·30	98·70	1 to 0·0132
Series II	1·60	1·26	97·14	1 „ 0·0130
Series I	3·58	1·19	95·23	1 „ 0·0125

The lowering is so small that it might readily be attributed to experimental errors, were it not that a precisely similar and much more strongly marked result is obtained with alloys of silver, zinc, and bismuth, as will be shown in a future paper.

After these limiting proportions are passed the amount of zinc

present increases with the silver present, giving sensibly the same curve in each series.

The above results obviously lead to the conclusion that silver and zinc form at least two definite compounds, viz., $AgZn_5$ and Ag_4Zn_5: the first of these can dissolve lead more freely than can either pure zinc or the second; so that when the silver and zinc are contained in the lighter alloy in exactly the proportion $AgZn_5$, the lead dissolved is a maximum. Moreover, it results conversely that lead can dissolve $AgZn_5$ more freely then either pure zinc or Ag_4Zn_5, so that, when circumstances favour the production of the first compound, the zinc dissolved in the heavier alloy is notably increased. It would seem that the compound Ag_4Zn_5 is so much less soluble than $AgZn_5$ that lead saturated therewith contains actually less zinc than when saturated with pure zinc; for the experiments with bismuth-silver-zinc alloys show that the zinc contained in the heavier alloys (calculated per unit of bismuth) gradually diminishes to a minimum as the silver present increases, and then regularly increases again, the position of this minimum being sensibly that where the silver and zinc present are in the proportion Ag_4Zn_5. This point will be discussed in a future paper.

When more silver is present relatively to zinc than corresponds with Ag_4Zn_5, then this compound is dissolved by lead, and conversely can itself dissolve lead, the more freely the more surplus silver is present; so that a more or less rapid rise in each solubility curve is observable when the silver present exceeds that requisite to form Ag_4Zn_5 with the zinc present.

The difference between the lighter alloy curves obtained in Series I and II leads to the remarkable conclusion that, when the compound $AgZn_5$ (containing dissolved lead) is kept fused for some hours, it tends to break up (presumably into free zinc and Ag_4Zn_5), and thereby to throw out of solution more or less of the dissolved lead; so that after 24 hours' fusion less lead is present in the lighter alloy than after only eight hours' fusion. Obviously, if it were practicable to effect this decomposition absolutely completely, the lighter alloy formed would be simply a mixture of Ag_4Zn_5 and more or less surplus zinc, saturated with lead; and hence the curve traced out with silver as abscissa and lead as ordinate should rise regularly from the origin up to the point where Ag_4Zn_5 without surplus zinc is present.

Owing to the volatility of zinc, we were unable to carry out any experiments with a view to tracing out such a regular curve, by maintaining the alloys in a fused state for lengthened periods of time (several days); but we succeeded in effecting the same object by the simple device of eliminating the lead that separated (along with some silver and zinc) on keeping the solution of lead in $AgZn_5$

fused for some time, so as to remove it from the sphere of action, and thus facilitate the separation in the same kind of way that removing the products of decomposition *pari passu* with their formation facilitates the decomposition by heat or the dissociation of ordinary chemical compounds.

After various trials, we found that the simplest way of effecting this was to prepare a series of mixtures of equal weights of lead and zinc and varying quantities of silver, and keep them fused for about eight hours, as in Series I; the compound ingots thus obtained were then cut in two, and the lighter portions fused separately without stirring for another period of eight hours, so as to be out of contact with the heavier portions formed during the first fusion. Similarly, the heavier portions were also fused separately. The result of this treatment was that each lighter portion underwent a further separation into a small quantity of heavy alloy and a much larger quantity of a lighter one; and, conversely, each heavier portion similarly separated into a small quantity of lighter alloy, which floated, and a much larger amount of heavier alloy. On repeating the operation, by cutting off the small quantity of heavy alloy (or lighter) that had thus separated, and fusing again for another eight hours, *no further separation to any material extent occurred* in the generality of cases, indicating that the limit of decomposition by fusion had been reached. Thus, for example, the following figures were obtained in two experiments with the lighter portions of the ingots first formed:—

Composition.	Silver.	Lead.	Zinc.
After first fusion for 8 hours	14·65	8·30	77·05
" second "	16·93	2·64	80·43
" third "	17·60	2·89	79·51
Mean of last two results	17·27	2·76	79·97
After first fusion for 8 hours	24·10	9·06	66·84
" second "	27·37	2·66	69·97
" third "	28·85	2·71	68·44
Mean of last two results	28·11	2·68	69·21

Similar results were obtained in various other cases; moreover, we found that, if instead of weighing up equal quantities of zinc and lead and a given proportion of silver, and fusing for eight hours, and then separating the two crude alloys formed, and again fusing the lighter alloy, a mixture of the three metals was made in about

the proportions representing those due to any given point on the earlier portion of curve No. 1, fig. 5, and kept in a state of fusion for eight hours or more, just the same effect was produced; *i.e.*, a small quantity of heavy alloy subsided, leaving a lighter alloy, *the composition of which was not altered materially by cutting off the separated heavy alloy and fusing again for eight hours more.*

Uniting together all the observations thus made, we obtained the following series of figures, representing the limiting compositions of the normal lighter alloys formed, *i.e.*, the compositions below which no further reduction in lead percentage could be obtained by keeping in a fused state for several hours longer:—

Series III.—Limiting Composition of Lighter Alloys.

Silver.	Lead.	Zinc.
11·51	2·37	86·12
17·27	2·76	79·97
24·49	2·65	72·86
28·11	2·68	69·21
37·54	3·14	59·32
46·23	3·56	50·21
52·78	3·60	43·62
61·12	6·40	32·48
64·58	10·00	25·42
66·25	10·84	22·91

On plotting these figures it is obvious that they give a curve (No. 3, fig. 5, dotted line) sensibly identical as regards its latter part with the corresponding portions of the curves obtained from Series I and II, the differences in no case being greater than the amounts that may reasonably be ascribed to unavoidable differences in the average temperature and in the small temperature fluctuations in the different experiments. But the first part of the curve is wholly different, the rise to a maximum at $AgZn_5$ and subsequent fall being wholly eliminated, and a regular ascent being visible instead, precisely as should be the case were the compound $AgZn_5$ wholly decomposed in each case, so that no excess of lead could be taken into solution over and above that due to the normal solubility in the particular mixture of silver and zinc (or of Ag_4Zn_5 and surplus zinc or silver) present. In similar fashion, we found that if the bottom portions of the compound ingots obtained after eight hours' fusion, and containing unduly high percentages of zinc, were cut off, and fused again separately for another period of eight hours, a small quantity of lighter alloy rose to the top, whilst the heavier alloy formed after the second fusion gave, on analysis, percentages of zinc and silver yielding a perfectly regular curve, instead of the abnormal

results obtained in Series I and II with mixtures of metals containing less than 28—41 per cent. of silver, and yielding heavier alloys after the first fusion, containing less than 5 or 6 per cent. of silver.

Series IV.—Limiting Compositions of Heavier Alloys.

Silver.	Lead.	Zinc.
0·38	98·33	1·29
0·75	97·98	1·27
1·52	97·30	1·18
4·03	94·72	1·25
8·44	90·00	1·56
11·09	87·20	1·71
14·82	83·25	1·93

The dotted curve No. 3, fig. 6, represents these values, which obviously imply that the method of treatment adopted had, as with the lighter alloys, sufficed to decompose completely the compound $AgZn_5$ originally present, and hence to prevent any larger proportion of zinc being present than that due to the normal solubility in lead of Ag_4Zn_5 mixed with free zinc, in the case of the alloys earliest in the series, or to the solubility in lead containing surplus silver of the compound Ag_4Zn_5, in those occurring later in the series.

The following tables are derived from all the foregoing results, excepting the earlier alloys of Series I and II, where excess of lead was present in the lighter alloy, or of zinc in the heavier one, owing to the presence of undecomposed $AgZn_5$; they represent the mean composition of the zinc-lead-silver alloys, producible under such conditions that no $AgZn_5$ is present to increase the amount of zinc contained in the heavier, and of lead in the lighter, alloys respectively :—

Solubility of zinc in lead-silver.			Solubility of lead in zinc-silver.		
Percentage of silver.	Percentage of zinc.	Difference for 1 per cent.	Percentage of silver.	Percentage of lead.	Difference for 1 per cent.
0	1·30		0	1·57	
1	1·25	−0·05	10	1·97	0·040
2	1·20	−0·05	20	2·37	0·040
3	1·20	0	30	2·77	0·040
4	1·22	+0·02	35	3·00	0·046
5	1·24	+0·02	40	3·25	0·050
6	1·26	+0·02	45	3·55	0·06
7	1·28	+0·02	50	3·90	0·07
8	1·32	0·04	51	3·97	0·07
9	1·38	0·06	52	4·04	0·07
10	1·46	0·08	53	4·12	0·08
11	1·56	0·10	54	4·20	0·08
12	1·68	0·12	55	4·35	0·15
13	1·81	0·13	56	4·60	0·25
14	1·95	0·14	57	5·00	0·40
15	2·10	0·15	58	5·5	0·5
16	2·26	0·16	59	6·1	0·6
17	2·43	0·17	60	6·8	0·7
18	2·61	0·18	61	7·55	0·75
19	2·80	0·19	62	8·35	0·8
20	3·00	0·20	63	9·2	0·85
			64	10·1	0·9
			65	11·1	1·0
			66	12·5	1·4
			67	14·5	2·0
			68	17·5	3·0

On comparing together the relative effects on the solubility of lead in zinc, and zinc in lead, produced by the presence of tin and silver respectively, it is at once obvious that if 100 parts of zinc can take up m parts of lead in presence of x parts of a third metal (tin or silver), or if 100 of lead can take up n parts of zinc in presence of x of the third metal, then m and n are invariably much greater when the third metal is tin than when silver, even if the solubility in presence of tin be reckoned at only 650° C., or thereabouts, instead of 800°.

Thus the following tables are calculated from the mean solubility curves previously described, giving the correlated values of x, m, and n for the three cases—tin at about 650°, tin at about 800°, and silver at about 800°:—

x.	Zinc dissolved by 100 parts of lead in presence of x parts of tin (or silver).						Lead dissolved by 100 parts of zinc in presence of x parts of tin (or silver).					
	Tin at 650°.		Tin at 800°.		Silver at 800°.		Tin at 650°.		Tin at 800°.		Silver at 800°.	
	n.	Diff.	n.	Diff.	n.	Diff.	m.	Diff.	m.	Diff.	m.	Diff.
0	1·25	..	1·32	..	1·32	..	1·15	..	1·60	..	1·60	..
5	1·90	0·65	2·60	1·28	1·32	0	2·05	0·90	3·7	2·1	1·85	0·25
10	2·55	0·65	4·00	1 40	1·70	0·38	2·95	0·90	5·8	2·1	2·10	0·25
15	3·6	1·05	5·55	1·55	2·25	0·55	3·85	0·90	7·9	2·1	2·35	0·25
20	5·1	1·5	7·35	1·80	2·90	0·65	4·90	1·05	10·0	2·1	2·60	0·25
25	7·2	2·1	9·70	2·35	3·70	0·80	6·4	1·5	12·1	2·1	2·85	0 25
30	9·9	2·7	12·6	2·9	..	..	8·4	2·0	14·2	2·1	3·10	0·25
35	13·3	3·4	16·2	3·6	..	..	10·9	2·5	16·35	2·15	3·35	0·25
40	17·1	3·8	20·3	4·1	..	..	13·8	2·9	18·50	2·15	3·60	0·25
45	21·2	4·1	24·9	4·6	..	..	17·0	3·2	20·7	2·2	3·85	0·25
50	25·6	4·4	29·8	4·9	..	..	20·6	3·6	23·0	2·3	4·10	0·25
55	30·2	4·6	35·0	5·2	..	..	24·5	3·9	..	..	4·40	0·30
60	34·9	4·7	40·4	5·4	..	..	28·7	4·2	..	..	4·70	0·30
65	39·7	4·8	45·9	5·5	..	..	..	..	..	..	5·05	0·35
70	44·6	4·9	51·5	5·6	..	..	..	..	..	..	5·40	0·35
75	49·6	5·0	57·2	5·7	..	..	..	..	..	..	5·80	0·40
80	54·7	5·1	..	..	..	..	..	..	..	..	6·20	0·40
85	59·8	5·1	..	..	..	..	..	..	..	..	6·60	0·40
90	65·0	5·2	..	..	..	..	..	..	..	..	7·00	0·40
95	70·3	5·3	..	..	..	..	..	..	..	..	7·40	0·40
100	75·6	5·3	..	..	..	..	..	..	..	..	7·80	0·40

FIG. 7.

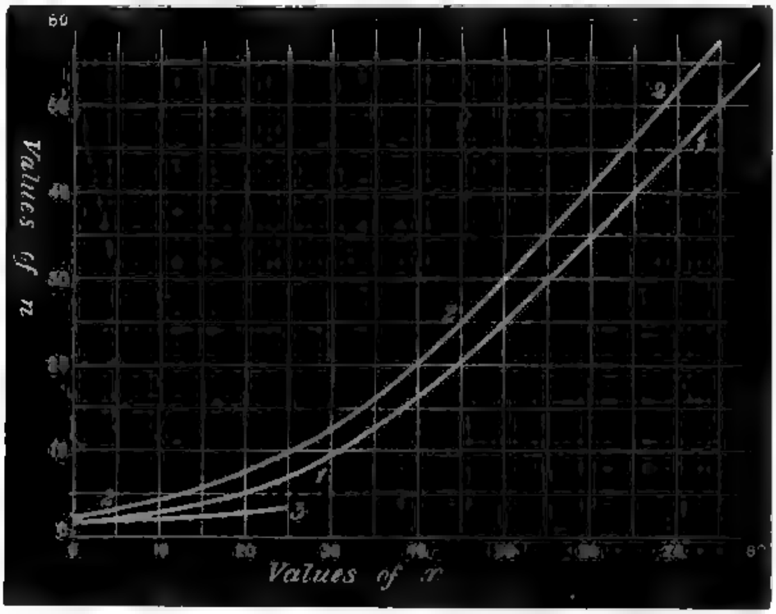
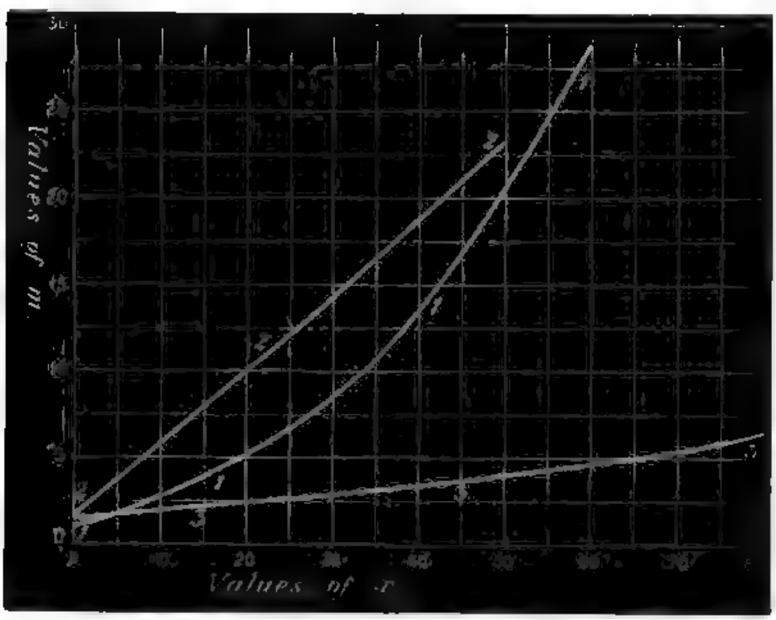


FIG. 8.



Figs. 7 and 8 represent these values of x, n, and m, respectively, the curves marked 1 being those obtained with tin at 650°; those marked 2 with tin at 800; and those marked 3 with silver at 800°; the value of x being abscissæ, and those of m and n ordinates.

We have much pleasure in acknowledging the assistance afforded us by Mr. T. M. Wyatt in carrying out a considerable portion of the analytical results above detailed.

II. "Experiments on Vapour-density." By E. P. PERMAN, B.Sc., Clothworkers' Exhibitioner at University College, London. Communicated by Professor RAMSAY, F.R.S. Received April 17, 1890.

The Vapour-density of Bromine.

The main purpose of my work on this subject was to discover if bromine had any tendency to dissociate at low pressures, *i.e.*, down to about 15 mm. of mercury, and at moderate temperatures.

The subject was suggested by Professor Ramsay, to whom I am greatly indebted for constant advice and assistance in carrying out the work. The method adopted for determining the vapour-densities was a modification of the Dumas method, from which it differed in three essential points:—(1) The globe was not immersed in a bath, but had a vapour-jacket. (2) The weight of the vapour in the globe was not found by direct weighing, but by running in an absorbent liquid, and estimating it volumetrically. (3) A series of vapour-density determinations at different pressures was made with the same quantity of vapour by lowering the pressure, absorbing the vapour drawn off and estimating its quantity.

As to previous work on this subject, Jahn has shown ('Wien, Akad. Sitzber.,' vol. 85, 2. Abth., 1882, p. 778) that the vapour-density becomes normal at about 230°; Meier and Züblin ('Deutsch. Chem. Ges. Berichte,' vol. 13, 1880, p. 405) and Crafts ('Comptes Rendus,' vol. 90, 1880, p. 183) have shown that partial dissociation takes place at very high temperatures; and Professor J. J. Thomson states that "vapour-density determinations showed that bromine vapour is dissociated if it is heated for a long time at a low pressure, even though the temperature is not very high" ('Roy. Soc. Proc.,' vol. 42, 1887, p. 345). His chief results are:—

Pressure.	Temperature.	Density.	Remarks.
mm.	°		
543	89	81·7	In bath 24 hours.
235	109	77·0	Sparked.
230	100	66·5	In bath 4 hours.
165	90	77·0	Only short time in bath.
390	111	70·0	In bath 7 hours.

These results were so abnormal that it was deemed necessary to make fresh experiments.

Description of Apparatus.—The apparatus used consisted of a glass globe, A (fig. 1), of about half a litre capacity, blown inside of a larger

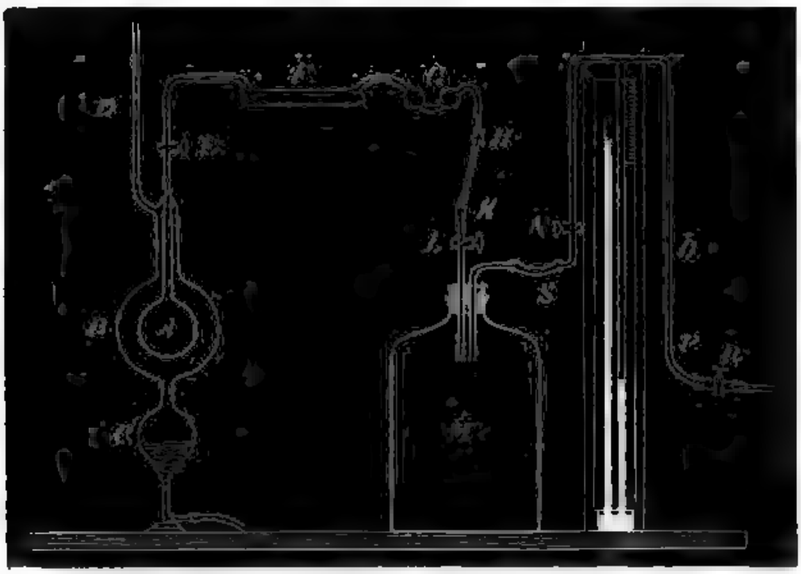


FIG. 1—Apparatus used for determining the Vapour-density of Bromine. (Section.)

globe, B. The liquid which produced the vapour-jacket was boiled in a small globe, C, connected with the outside globe B, and condensed in the tube D, round which, when necessary, a condenser was fixed. Further to the right came an absorption-tube, F, with a small bulb-tube, G, containing a few drops of the same liquid as was introduced into the absorption-tube, viz., potash solution during the boiling-out process, and afterwards a strong solution of potassium

iodide; then came an air-reservoir, M, and a pressure-gauge, O, arranged as shown in the diagram. There were stopcocks at E, L, and N; rubber joints at H, K, and S; and a screw-clip at R on the tube P, leading to a water-pump.

Method of Procedure.—The capacity of the globe A was accurately determined by weighing the apparatus empty, and then full of water; it was filled from a wash-bottle, which was also weighed as a check on the other result.

Pure bromine was specially prepared by boiling commercial bromine with potassium bromide for a day, distilling it through a tube containing red-hot manganese dioxide, and redistilling with phosphoric anhydride; after a small portion had distilled over, the boiling point was constant at 59°·09 (reduced to 760 mm.). Asbestos plugs were found very useful in working with bromine; by their use a good joint can be secured if *liquid* bromine is not allowed to come into contact with the asbestos. The thermometer in the distilling-flask was secured by a plug of asbestos, and its bulb was surrounded with asbestos to prevent superheating.

To commence an experiment, the globe was rinsed with ether, and dried by repeated exhaustion and admission of air; some bromine was run in without admitting air, the tube above E was fused on to the absorption-tube, F, by means of a portable blowpipe, the liquid in C was boiled, the stopcock E cautiously turned till the pressure within the globe was equal to the atmospheric pressure, and then opened from time to time till the bromine ceased to blow out. All the bromine was absorbed in F, which at this stage contained potash solution. The stopcock E and the tube above and below it were gently heated by means of a Bunsen's burner. The absorption-tube was then connected with the reservoir M, and the air was partially exhausted from the whole apparatus; to complete the exhaustion as far as possible, the globe and the absorption-tube were directly connected with the pump. The stopcock E was then turned off, the tube above it was cracked and removed, more bromine was admitted, the tube fused together again, and the bromine driven out as before till the pressure in A was equal to the atmospheric pressure. The globe was then full of bromine vapour at the atmospheric pressure, and at the temperature of the jacketing vapour, and was ready for a vapour-density determination, or for a series of determinations. The absorption-tube was rinsed out, and partly filled with a strong solution of potassium iodide; it was then clamped in its place, and connected with the globe A, and with the reservoir M. The water-pump was then set to work till the reservoir was exhausted sufficiently—the degree of exhaustion depending upon the number of vapour-density determinations which were to be made in the series. The pump was then cut off by the screw-clip R, and

he stopcock E turned very cautiously, letting out the bromine vapour little by little; when it ceased to come over, the globe and its contents were left for two or three minutes in order to regain the heat lost by the adiabatic expansion; it was necessary to repeat this process till the bubble in the bulb-tube G remained undisturbed on quickly opening and closing the stopcock E. The pressure recorded by the gauge was then carefully read, the stopcock L was closed, the absorption-tube removed, and its contents washed into a stoppered bottle. The whole process was repeated at successively lower pressures till the lowest desired was reached; a good water-pump will reduce the pressure to the vapour-pressure of water at the temperature of the water passing through the pump.

The residual bromine in the globe was estimated by running in potassium iodide solution, and titrating with a standard solution of sodium thiosulphate. The successive quantities of bromine collected in the absorption-tube were also estimated in the same way. The thiosulphate solution was standardised with the specially prepared bromine, and also with pure iodine; the results agreed within 0·2 per cent., and the mean value was used.

By adding together the residual quantity of bromine and the quantities removed, the weight of bromine in the globe at each pressure was found. The capacity of the globe being accurately known, and also the temperature of the jacket from the tables of Professors Ramsay and Young ('Chem. Soc. Journ.,' vol. 47, 1885, p. 640), the data requisite for the determination of a series of vapour-densities at different pressures were available.

Results.—After some practice in conducting the experiment, satisfactory results were obtained; they are tabulated below:—

Series I.—Temperature, 77°·8 (Alcohol); Capacity of Globe, 446·3 c.c.

	Weight of bromine.	Pressure.	V.-d.
	grms.	mm.	
1.	2·410	744·9	79·2
2.	1·856	575·9	78·9
3.	1·383	431·2	78·5
4.	1·146	355·9	78·8
5.	0·6732	208·4	79·1
6.	0·3388	103·6	80·1
7.	0·1144	33·9	82·6

Some bromine escaped absorption, causing the first vapour-densities to be too low.

Series II.—Temperature, 78°·4.

	Weight of bromine.	Pressure.	V.-d.
	grms.	mm.	
1.	2·640	764·3	81·0
2.	1·552	452·0	80·5
3.	0·4848	141·4	80·4
4.	0·05933	15·45	90·1

The only noticeable point in this series is the highness of the last vapour-density at the lowest pressure.

A single experiment was then made in order to try the effect of continued heating on bromine vapour at a low pressure. After three hours' heating at 78°·0 and 15·35 mm. pressure, the vapour-density came out abnormally high, viz., 92·6. Professor J. J. Thomson found vapour-densities 66·5 and 70 at much higher pressures and somewhat higher temperatures than these.

Series III.—Temperature, 155°·5 (Bromobenzene); Capacity of Globe, 447·1 c.c.

	Weight of bromine.	Pressure.	V.-d.
	grms.	mm.	
1.	2·114	749·0	80·8
2.	1·192	423·9	80·3
3.	0·3309	117·4	80·7
4.	0·09833	34·9	80·4

Here there is still no sign of dissociation, and what seems somewhat remarkable, even at low pressures the vapour-density is above the normal density.

The pure bromine being then exhausted, a fresh sample was prepared from the residues and from some commercial bromine treated with potash; the whole was evaporated to dryness, ignited, distilled with sulphuric acid and potassium bichromate, and dried by shaking with sulphuric acid, allowing to stand, and re-distilling; the greater portion came over at 58°·9 (corrected to 760 mm.), it commenced to boil at 58°·55, but the boiling point soon rose to 58°·9, and then remained constant.

A determination was then made of the density of bromine vapour as nearly as possible saturated at 15°. The globe was immersed in

a pan of water, and the pressure was made so near the saturation-pressure that the bromine took three hours to vaporise completely.

Series IV.—Temperature, 14°·7—15°; Capacity of Globe, 445·6 c.c.

	Weight of bromine.	Pressure.	V.-d.
	grms.	mm.	
1.	0·4125	98·65	80·3
2.	0·2661	63·70	80·4
3.	0·1355	32·10	81·3
4.	0·07205	16·65	83·3

The vapour-pressure of bromine at 15° is 138·1 mm., according to the tables of Professors Ramsay and Young ('Chem. Soc. Journ.,' vol. 49, 1886, p. 445). These results show that, on approaching the liquid state, bromine has no tendency to form molecules with more than 2 atoms. This agrees with the results of Paternò and Nasini ('Deutsch. Chem. Ges. Ber.,' vol. 21, 1888, p. 2153) by Raoult's method, which indicated molecules Br_2 in aqueous and acetic acid solutions.

Series V.—Temperature, 279°·5 (Bromonaphthalene); Capacity of Globe, 448·6 c.c.

	Weight of bromine.	Pressure.	V.-d.
	grms.	mm.	
1.	1·625	744·7	80·0
2.	0·5072	233·2	79·8
3.	0·2138	98·1	80·0
4.	0·1082	49·45	80·3
5.	0·04939	21·85	82·9

The temperature employed in this series was much higher than the temperatures used by Professor J. J. Thomson in his experiments; some of the pressures were much lower, and the total time of heating was about five hours, yet there was no sign whatever of any dissociation.

Finally, an experiment was made with bromine in presence of air. The bromine was boiled out of the globe at exactly half the atmospheric pressure, and air was then slowly admitted until the pressure inside and out was the same.

Series VI.—Temperature, 132°·2 (Chlorobenzene); Capacity of Globe, 509·2 c.c.; Volume occupied by Bromine Vapour, 254·6 c.c.

	Weight of bromine.	Pressure.	V.-d.
	grms.	mm.	
1.	1·234	761·4	80·3
2.	0·6379	392·3	80·5
3.	0·1953	120·0	80·6
4.	0·08763	53·8	80·7
5.	0·03972	23·0	85·5

The results are negative, as before, but no doubt dissociation would be produced if the temperature were carried high enough, and I hope to carry on my experiments with a modified form of apparatus until dissociation takes place. I must ask my fellow-workers to kindly leave to me the completion of this work.

Remarks.—One of the most striking features in the series of vapour-densities is the invariable increase in density at low pressures; this may be accounted for partly by the error unavoidable in reading low pressures, and partly by the irregularity in the position of the liquid in the bulb-tube G; but these errors together would never amount to more than 1 mm. of mercury, seldom so much. As the chief cause of the anomaly, I would suggest that a film of bromine may adhere to the glass, and thus cause the residual amount of bromine to be too large.

Vapour-density of Iodine.

Previous Work.—V. Meyer obtained vapour-densities closely corresponding to a molecule $\frac{2}{3}I_2$, at a temperature which he estimated at 1570° ('Deutsch. Chem. Ges. Ber.,' vol. 13, 1880, p. 394).

Crafts and Meier found a greater amount of dissociation than V. Meyer at about the same temperature, which, however, they estimated at 1390° instead of 1570°; the lowest vapour-density they obtained was 75·6 ('Comptes Rendus,' vol. 90, 1880, p. 690).

Deville and Troost found the vapour-density normal at 860° and 1034° ('Annales de Chimie,' vol. 58, 1860, p. 257). Crafts and Meier, in further experiments, obtained complete dissociation of iodine vapour, and they give a curve showing the amount of dissociation at different pressures and temperatures ('Comptes Rendus,' vol. 92, 1881, p. 39). Crafts obtained partial dissociation by a modification of V. Meyer's method ('Comptes Rendus,' vol. 90, 1880, p. 183). Troost obtained a very considerable amount of dissociation by the Dumas method at 1250°; the lowest number obtained was 81·6; he also found the

vapour-density at 440° and 34·5 mm. pressure to be 106·2 ('Comptes Rendus,' vol. 91, 1880, p. 54). The dissociation of iodine has thus been worked out with fair completeness, although the results of

FIG. 2.

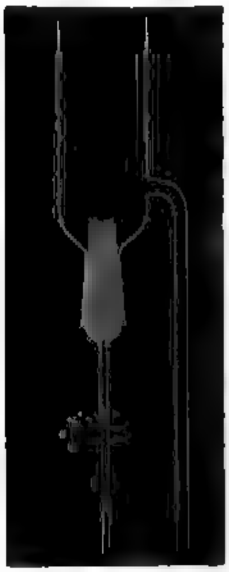


FIG. 3.

Forms of Apparatus used for Iodine and Aqueous Hydrochloric Acid. (Section.)

Deville and Troost seem very much at variance with those of other experimenters. The object of my work was to discover the density of iodine vapour as nearly as possible saturated. A modified form of apparatus was devised by Professor Ramsay so that the iodine could be conveniently introduced into the globe. The modification consisted of the introduction of a stopper at the neck of the globe with a capillary tube through the centre. The method of procedure was the same as with bromine, except that it was necessary to heat the

Temperature, 131°·4 (Chlorobenzene); Capacity of Globe, 517·6 c.c.

	Weight of iodine.	Pressure.	V.-d.
	grms.	mm.	
1.	0·6762	129·8	126·7
2.	0·2274	39·5	140·0

tube and stopcock next the globe to a much higher temperature, in order to volatilise the iodine; consequently there was great risk of cracking the stopcock, and this indeed happened several times. The first (trial) series gave results which pointed towards normal density.

In the next series an attempt was made to find the vapour-density as nearly as possible at the saturation-pressure. The vapour-pressure of iodine at 132° is 164·6 mm., from the results of Professors Ramsay and Young ('Chem. Soc. Journ.,' vol. 49, 1886, p. 455), and several determinations were made at about this temperature and at about 150 mm. pressure. Constant results were not obtained, although much time and trouble were expended on the experiments. The numbers obtained were:—247·6, 251·2, 204·5, and 184·9. The iodine was allowed to blow out until none could be seen on the bottom of the globe, but it was extremely difficult to decide whether it had all volatilised or not, and no reliance can be placed on the results, notwithstanding the close agreement of the first two numbers with 254 (= 2 × 127). Another plan was then adopted.

Vapour-density of Iodine from Determinations of the Velocity of Sound by Kundt's Method ('Poggendorff's Annalen,' vol. 135, 1868, p. 337).

The apparatus is best understood from the diagram (fig. 4). The glass tube CD, about 2 feet long and ¾ inch in diameter, was supported inside a larger tube, GH, by corks, as shown; CD was slightly

FIG. 4.—Section of Apparatus.

inclined one way, and GH the other. The bulb E contained chlorobenzene; F is a condenser; AB is a small-sized tube sealed into the tube CD at C; it was enlarged at B, so as to fit the tube loosely, but not to touch the walls.

Method of Procedure.—Some solid powdered iodine was introduced

into the tube CD, and shaken down to the end C. Some finely-divided precipitated silica was then introduced, and distributed along BD. The air was expelled by heating the tube with a Bunsen's burner, at the same time exhausting it by means of a water-pump; the end D was then sealed off. The tube CD was then placed inside the wide tube GH, as shown in the figure; the chlorobenzene was boiled, and when the whole tube had been heated for a few minutes, the projecting tube AC was rubbed with a cloth moistened with alcohol. The whole tube AB vibrated, the iodine vapour in BD was thrown into vibrations, and the silica on the lower part of the tube formed little heaps at the nodes, and streaks perpendicular to the length of the tube at the loops. The tube was then allowed to cool, withdrawn from the larger tube, and placed on a horizontal millimetre scale (an etched mirror scale was used). It was easier to determine the position of the loops than of the nodes when silica was used. The mean distance between two loops was then calculated. Similar experiments were made with air.

Let l_1 = mean distance between two loops for air.

l_2 = " " iodine vapour.

d_1 = density of air.

d_2 = " iodine vapour.

$k_1 = \dfrac{\text{sp. heat at const. press.}}{\text{sp. heat at const. vol.}}$ for air.

k_2 = " for iodine vapour.

Then it follows from the laws of the propagation of sound that

$$d_2 = \frac{l_1^2}{l_2^2} d_1 \frac{k_2}{k_1}.$$

Results.—l_1 was found to be (1) 35·4, (2) 35·8, (3) 35·7, eleven readings being taken in each experiment. The mean of (2) and (3) was taken as the correct value of l_1, these being the most trustworthy experiments. The results obtained for iodine vapour are here tabulated:—

	l_2.	Number of readings.	V.-d.
1.	15·23	11	132·0
2.	14·94	12	137·4
3.	11·68	10	126·3
4.	11·52	10	129·7
5.	11·75	9	124·7

Experiments (1) and (2) are not reliable, as the vibrating glass tube gave a different note from that produced in the experiments on air; it gave one of two notes, according to the way in which it was clamped. The interval between these notes was estimated by ear as a fourth, but it could not be determined accurately owing to the highness of the notes, which appeared also to vary slightly in pitch. The mean of experiments (3), (4), and (5) is 126·9. In the calculation, k_1 was taken as 1·405 (Röntgen), and k_2 as 1·31 (Strecker). These results show conclusively that saturated iodine vapour, and therefore liquid iodine also, are composed of diatomic molecules, I_2. This result agrees with the late work of Ernst Beckmann ('Zeitschrift für Physikal. Chem.,' vol. 5, 1890, p. 76) on the molecular weight of iodine in solution in ether and carbon bisulphide; the numbers varied from 235 to 261 for solutions in ether, and from 263 to 283 for solutions in carbon bisulphide.

A single experiment was performed with bromine in the same manner; the mean distance between the heaps of silica was 12·22 mm. at a temperature of about 29°. Compared with air at the same temperature, this gives a vapour-density 85·05. This points to a normal density, as would be expected. There was considerable uncertainty as to the temperature, because no heaps were formed while the bromine vapour was saturated, and the tube was therefore heated with a Bunsen's burner. Iodine vapour refused to give heaps in the same way when left long enough in contact with liquid iodine to become saturated. It was predicted by Professor Fitzgerald, in a private letter to Professor Ramsay, that sound would not be propagated through saturated vapour, because the rate of propagation of a compression would not be the same as that of an expansion, owing to the difference in elasticity in the two cases. Either the expansion or the compression of a sound-wave would produce condensation in any saturated vapour; in the case of saturated steam, *expansion* causes condensation, except at high temperatures (above the "temperature of inversion"). At present there are not sufficient data to calculate the effect of a wave of compression or expansion upon saturated iodine vapour; but condensation would take place either during a compression or an expansion, and the ratio dp/dv—and therefore the elasticity, $v\,dp/dv$—would be quite different in the two cases. The sound-waves would interfere with one another, and become confused, so that no sound would be propagated through the vapour.

Induction Spark through Iodine Vapour.—An experiment was then made to determine if the passage of the induction spark through iodine vapour effected its dissociation. Professor J. J. Thomson obtained vapour-densities 137 and 130 for unsparked iodine, and 110, 115, 84, and 86 for sparked iodine, the last vapour-density being determined twenty-four hours after sparking. Professor Thomson

says ('Roy. Soc. Proc.,' vol. 42, 1887, p. 344), "The appearance of the dissociated iodine is not greatly different from that of the unsparked; its colour, however, is, I think, a little lighter, and not so uniform. I was not able to detect any change in the absorption-spectrum produced by the sparking. The electric strength of the sparked gas was, however, less than that of the unsparked." The same apparatus was used, platinum wires being fused into the glass (see fig. 4). The jacket used was methyl salicylate vapour, temperature 223°·5; the iodine was boiled out at the atmospheric pressure, and the tube then sealed at D. The mean distance between the heaps without sparking was 14·24 mm., and after sparking for fifteen minutes, and cooling for three minutes (to allow the vapour to regain the temperature of the jacket), the mean distance was 14·3 mm. Another experiment was then made, so that the heaps were formed *during* the sparking; the mean distance between the heaps was 14·2 mm. on the side of the piston near the vibrator, and 14·4 mm. on the other side; the difference is probably due to the heating effect of the induction sparks.

The results may be tabulated thus:—

Mean distance between heaps.		
Before sparking.	*After* sparking.	*During* sparking.
14·24	14·3	14·2
		14·4

Suppose that complete dissociation of the iodine vapour took place on sparking, then the pressure would be doubled, and therefore the elasticity would be doubled; but the density (taking no account of the pressure) would remain unaltered.

Let v_1 = velocity of sound in the undissociated iodine vapour.
v_2 = " completely dissociated ditto.
e = elasticity of the undissociated vapour.
d = density of the iodine vapour (same in both cases).

Then
$$v_1 = \sqrt{\frac{e}{d}},$$
and
$$v_2 = \sqrt{\frac{2e}{d}}.$$

But the distance between the heaps of silica (half a wave-length) is directly proportional to the velocity of sound in the vapour; therefore, if any dissociation took place, there would be a marked increase in the distance between the heaps on sparking.

The results given above agree within the limits of the errors of

experiment, and it is concluded, therefore, that sparking does not produce any permanent dissociation, although very probably a momentary dissociation is caused near the terminals when a spark passes. The length of the spark in the iodine vapour was about 1 inch; the coil was capable of giving a 3-inch spark in air. By opening the tube under water, it was found to contain about 5 per cent. of air; this would tend to aid dissociation, and would in no way lessen the value of the experiment.

The probable experimental error was about 1 per cent., and was caused by the difficulty in reading the distance between the loops as indicated by the arrangement of the silica.

Vapour-density of Sulphuric Anhydride.

The apparatus used to determine the vapour-density of sulphuric anhydride was similar to that used for iodine. The only work that has been done on this subject is apparently that by Schultz-Sellack ('Poggendorff's Annalen,' vol. 139, 1870, p. 480) and Mitscherlich ('Watts's Dictionary'), who both found the vapour-density normal. Some of the trioxide was introduced into a small flask, the neck of which was then sealed on to the tube from the inner globe; it was then sublimed into the globe, which had been previously exhausted, by heating the flask. The vapour-density was then determined in the usual way. The trioxide was absorbed by water, and estimated by means of a standard ammonia solution. Using cochineal as indicator, it was found possible to titrate accurately to one drop of a decinormal solution in the following way:—The relative value of seminormal (approximately) solutions of acid and ammonia was found, and these solutions were then diluted to decinormal strength; a neutral solution was then made by using decinormal acid and ammonia in the proportion found for the seminormal solutions, and this was used as a standard neutral solution, with which the solutions titrated were compared. The chief difficulty was the formation of sulphuric acid above the stopcock; it was partially obviated by introducing a horizontal bulb-tube above the stopcock—this retained most of the acid. Only one series of experiments was made; the first result is very nearly at saturation-pressure.

	Temperature.	Weight of SO_3.	Pressure.	V.-d.
	°	grms.	mm.	
1.	22·1	0·1281	56·7	40·9
2.	22·7	0·08754	40·5	39·2
3.	22·8	0·04876	22·1	Calc. normal.

It was useless to estimate the residual amount of trioxide, as sulphuric acid had collected in the lower part of the globe while the trioxide was being sublimed into it. The vapour-density at the lowest pressure was therefore assumed to be normal (corresponding to SO_3), as it would be in all probability, and on this assumption the other vapour-densities are also normal. If the third vapour-density were actually above normal, the second would be less above normal, and the first still less; while, if the third vapour-density were actually below normal, the others would also be below normal; these alternatives are very improbable, and I regard it as fairly proved that the formula of sulphuric anhydride is SO_3, and not S_2O_6.

Vapour-density of Aqueous Hydrochloric Acid.

Commercial hydrochloric acid was distilled until the boiling point became constant, at 108·2° (pressure, 745 mm.). The distillate was then collected and used for vapour-density determinations; to find the percentage of real acid, weighed quantities were titrated with a standard soda solution. The acid used in Series I contained 20·82 per cent. HCl; that in Series II, 21·33 per cent.; and Series III, 20·45 per cent. The last two samples were distilled from pure acid. The vapour-density was found by means of the apparatus used for iodine. The acid coming over on lowering the pressure was absorbed by a standard soda solution in the absorption-tube; much of it, however, condensed above the stopcock, and caused some difficulty.

Results.

Series.	Temperature.	Capacity of globe.	Weight of HCl and H_2O.	Pressure.	V.-d.	Theoretical V.-d.
	°	c.c.	grms.	mm.		
	131·2	509·2	0·3004	741·8	10·01	10·06
I.	132·7	649·2	0·3502	773·5	8·81	”
			0·3038	575·1	10·30	”
			0·1495	324·4	8·96	”
			0·06035	126·5	9·28	”
II.	132·2	537·0	0·3204	762·0	9·88	10·09
			0·2349	580·9	9·50	”
			0·1742	428·7	—	”
			0·07778	191·4	9·54	”
III.	131·5	354·9	0·2134	747·8	10·12	10·04
			0·1621	573·8	10·02	”
			0·1027	357·1	10·20	”
			0·04629	146·4	11·22	”

There was some loss in Series I and II in removing the acid from above the stopcock; this was done by aspirating air through the tube, which was heated by a Bunsen's burner, and absorbing the acid in a soda solution; but an acid fume was formed, which refused to be absorbed. In Series III, the apparatus with the jacketed stopcock was used, and the acid which collected above the stopcock was washed out. The vapour-density in the extreme right-hand column is calculated for a mixture of hydrochloric acid and water in the proportions found by titration. These results show that no compound of acid and water is formed, at least at the temperature employed; they confirm the results of Bineau, who found a vapour-density 10·04 at the atmospheric pressure ('Annales de Chimie,' vol. 7, 1843, p. 257).

III. "On Barometric Oscillations during Thunderstorms, and on the Brontometer, an Instrument designed to facilitate their Study." By G. J. SYMONS, F.R.S. Received April 24, 1890.

The fact that a rise of the barometer occurs during thunderstorms has been supposed by many to be newly discovered through the general establishment of self-recording barometers; but Dr. Hellmann has shown that it was noticed by J. J. Planer as far back as 1782. In 1784, Rosenthal epitomised the facts as follows:—"When a thunderstorm approaches the place where a barometer is situated, the mercury in the tube begins to rise; the nearer the thunder-cloud comes to the zenith of the observer, the higher does the mercury rise, and it reaches its highest point when the storm is at the least distance from the observer. As soon, however, as the cloud has passed the zenith, or has become more distant from the observer, the weight of the atmosphere begins to decrease and the mercury to fall." A few years later, Toaldo determined the amount of the rise in several storms, and found it to be between 1 and 2 lines (0·09 in. to 0·18 in.).

Professor Strehlke (in 1827–30) made several sets of observations, and found the rise to be from 0·04 in. to 0·06 in., and was probably the first to point out that the highest point of the barometer is not absolutely synchronous with the passage of the centre of the storm-cloud, but seems rather to be always at a certain distance from it.

Kaemtz, in his 'Lehrbuch' (1832), suggests that the rise is produced by the inrush of air towards the site of the storm, this accumulation causing the rise of the barometer as the storm nears the zenith.

Although Luke Howard had a recording barometer at work in the early part of this century, he seems to have failed to notice the

phenomenon; and no one in England seems to have been aware of it until the photographic barometer was started at the Radcliffe Observatory, Oxford. Manuel Johnson, when describing the new instruments at the British Association meeting at Glasgow, in 1855, said:—

"Among the most remarkable results is a sudden rise of the barometer, amounting to 0·035 in., and an increase of temperature of 1°, coincident with the occurrence of a thunder clap which struck one of the churches in Oxford, July 14th, 1855. A similar phenomenon took place during a thunderstorm on August 23rd, when the rise of the barometer was still greater, amounting to 0·049 in., though the thunder clap coincident with this rise was distant."

Mr. Johnson returned to the subject in the volume of 'Radcliffe Observations' for 1857, and gave reproductions of fourteen barograms, but the scale is so compressed (only $\frac{1}{4}$ in. per hour, and $1\frac{1}{2}$ in. per inch of mercury) that not much is to be learned from them beyond the fact of falls occurring of 0·037 in., 0·040 in., and 0·046 in., and a rise of 0·070 in.; the notes on the storms are also too vague to be useful. It may, however, be well to quote the conclusion at which Mr. Johnson arrived, viz.:—

"A comparison of these notes with the accompanying illustrations cannot, in my opinion, fail to lead to the inference that the disturbances exhibited both on the barometric and the thermometric curves (especially the former) are caused by the presence of electricity in the atmosphere, of which we had on these occasions sensible proof. But they are the more interesting, from the circumstance that similar disturbances occur not unfrequently when there has been no overt manifestation of that agency; especially during the winter months, when, according to the concurrent testimony of all observers, atmospheric electricity is most abundant."

The next observation of importance is one quoted by Le Verrier, as reported to him by the observer, M. Goullon, Curé of Saint-Ruffine (Moselle). He had two barometers, a mercurial and an aneroid. On the morning of February 5th, 1866, the weather being stormy with heavy rain, wind S.W., moderate, but not squally, he had just set and read his barometers, when there was a solitary loud clap of thunder, and instantly both his barometers rose 2 mm. (0·08 in.).

The Hon. Ralph Abercromby began studying these oscillations in 1868, and in 1875 summed up the results in the following sentences, one descriptive, the other explanatory:—

"There are two classes of storm in this country: in the one the barometer rises, in the other it falls. In the case in which it rises, the sequence of weather is somewhat as follows:—After the sky has become overcast, the wind hushed to an ominous silence, and the clouds seem to have lost their motion, the barometer begins to rise suddenly.

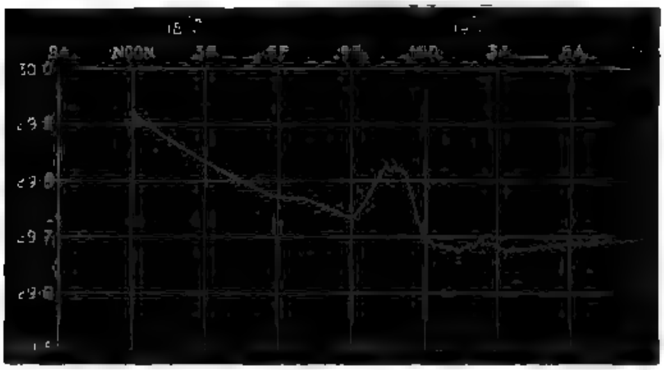
In the middle of this rise, sudden heavy rain begins. After a few minutes the rain, with or without thunder and wind, becomes a little less heavy, and the barometer sometimes falls a little. The rain then continues till the end of the squall, and as it stops the barometer returns to its original level. In Great Britain the rise rarely exceeds 0·10 in., or lasts more than two hours. These rises are always superadded to a more general rise or fall of the barometer, due either to a cyclone or to one of the small secondaries which are formed on the side of one. During some rises the wind remains unchanged; with others there is a more or less complete rotation of the wind. In all cases the disturbance seems to be confined to the lower strata of the atmosphere." . . . . . "Since the rise is always under the visible storm, it is propagated at the same rate and in the same manner as the thunderstorm. Enough is known of the course of the latter for it to be certain that they are not propagated like waves or ripples, and hence these small barometric rises are not due to aerial waves, as has been suggested. Since their general character is the same whether there is thunder or not, it is evident that electricity, even of that intensity which is discharged disruptively, is not the cause of the rise. If we look at a squall from a distance, we always see cumulus above it, which is harder or more intense in the front than in the rear of the squall. Since cumulus is the condensed summit of an ascensional column of air, it is evident that the barometric rise takes place under an uptake of air. If we consider further that a light ascensional current would give rise simply to an overcast sky, a stronger one to rain, while a still more violent one would project the air suddenly into a region so cold and dry that the resulting electricity would be discharged disruptively as lightning, the foregoing observations show that the *greatest rise is under the greatest uptake.* Our knowledge of the mechanics of fluid motion is still too unsettled for us to say with certainty whether or not an ascensional current of air would have a reaction backwards, like a jet of air issuing from an orifice."

Professor Mascart also, in 1879, expressed the opinion that electricity had nothing to do with these oscillations, but suggested quite a different explanation. Premising that they are not produced by all heavy rains, but only when heavy showers fall during bright weather, he suggested that at such times rain falls through non-saturated air, where it would evaporate freely, and so produce a local increase of pressure which in certain thunder-rains might amount to 2 mm. (0·08 in.). He explained the diminution of pressure which sometimes occurs, by the reversed phenomenon; he considered that thunderstorms are formed locally, and suggested that the condensation of masses of vapour into rain drops ought to produce a diminution of pressure.

M. Teisserenc de Bort and Mr. Budd have suggested that the rise may be due to the local compression of the air by the multitude of falling rain drops.

In the 'Annales' of the French Meteorological Office for 1880, M. Renou gives reproductions of some barograms from the Observatory at Parc St.-Maur. A very interesting one is reproduced in fig. 1. M. Renou does not append any remarks to the plate; but

Fig. 1.



Parc St.-Maur, Paris, August, 1878.

from other sources it appears that there was a heavy thunderstorm from 10 till 11 P.M. on August 18th, 1878. The total rise may be taken as 0·10 in. and the fall as nearly 0·15 in.

Dr. Fines, of Perpignan, established a Redier barograph in 1875, and in a memoir published in 1883 gives reproductions of the traces during several storms. He states that, considering the present imperfect knowledge of the real conditions and origin of thunderstorms, it seems useless to try to explain records frequently influenced by very distant storms. One fact, however, is certain, that thunderstorms are accompanied by great condensation, which must cause variations in the density of the air, and therefore should affect the barometer. He gives 0·144 in. in 20 minutes as the greatest variation, but says that it rarely exceeds 2 mm. or 3 mm. (0·08 in. or 0·12 in.) in an hour. The only facts which are certain from the records at Perpignan are that there is usually (1) before heavy rain, decrease of pressure and temperature; (2) with the rain, sudden increase of wind, rapid rise of barometer, and fall of temperature; (3) at the end of the storm-rain, reversal of the last three phenomena.

Dr. Ciro Ferrari, who has devoted great attention to the progress

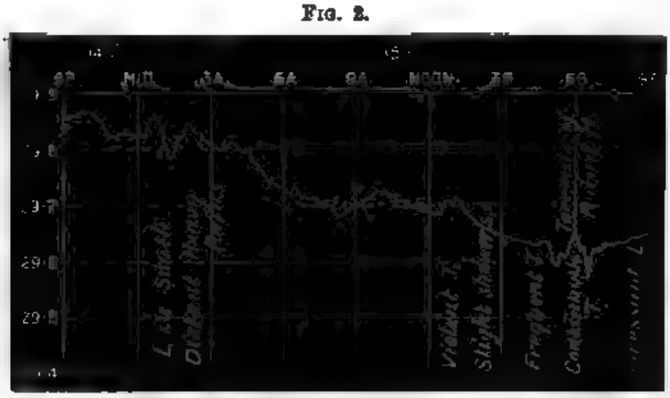
and character of the thunderstorms of North Italy, considers the rise due to secondaries on the skirts of cyclones.

Professor Börnstein regards them as due to temperature changes not reaching to the upper strata of the atmosphere, in which view he has partly the support of Professor Ferrel.

Professor Klossovsky, of Odessa, says that, "Every storm, whether with or without hail, is accompanied by barometric oscillation." If by this phrase he means oscillations like those observed in other parts of Europe, the phenomena must be different in Southern Russia, for one of the difficulties in London, for example, is that they do not occur with *all* storms, but only with *some*.

In the 'Annuaire' of the Montsouris Observatory for 1889, M. Descroix, when referring to the subject, gives a barogram for August 15th, 1888, which, with the accompanying notes, is so typical that it is reproduced (reduced to the same scale as the others) in fig. 2.

Fig. 2.



Montsouris, Paris, August, 1888.

It will be noticed that several of the opinions above quoted are contradictory, and it is not known that any one explanation is generally accepted.

The author was much struck by the remarkable curve given by his Redier barograph on the evening of August 2nd, 1879, and still more so when he found that the curve from a similar instrument at Messrs. Lund and Blockley's, in Pall Mall, 2⅜ miles S. by W. from his own instrument, was nearly identical (figs. 3 and 4), and he has long desired to investigate the subject. At last, in 1886, he definitely decided upon the phenomena which he considered it necessary to record mechanically. The author had read the description of Sir Francis Ronalds's storm clock, and the objects being similar, that

Fig. 3.

Camden Square, London, August, 1879.

Fig. 4.

Pall Mall, London, August, 1879.

description was useful, though the completed apparatus has scarcely one feature which even resembles Sir Francis's.

Before describing the instrument, the author considers it only fair to MM. Richard Frères, to state that not only has the apparatus been constructed, but almost wholly invented, by them; the author decided upon what the instrument was to do, and upon the scales required, but he left the whole constructional details to MM. Richard, and considers the result a great credit to the firm.

As the primary object of the instrument* is the study of the phenomena of thunderstorms, it has, for brevity, been termed a Brontometer, or Thunderstorm-measurer.

* Other uses for it are already apparent.

It is provided with endless paper, 12 in. wide, travelling under the various recording pens at the rate of 1·2 in. per minute, or 6 ft. per hour. This is about 150 times faster than is usual in meteorological instruments, and enables the time of any phenomenon to be read off with certainty to a single second of time.

The traces are made in aniline ink by a series of seven Richard pens.

The first pen is driven by the clock which feeds the paper, so that the time scale and the paper must go together. The pen usually produces a straight line, which serves as the base line for all measurements, but at 55 seconds after each minute the pen begins to go, at an angle of about 45°, one-tenth of an inch to the left, and at the sixtieth second it flies back to its original position.

The second pen is driven by one of Richard's Anemo-cinemographs, a name which they have given to a pattern of anemometer not yet known in England. The external portion has some resemblance to the ordinary windmill governor, but it differs from it in that the plates are curved, not flat; they are made of aluminium, and are so light that they have little momentum, and have thus a great advantage over cups, which run on for many seconds after the wind force has decreased or ceased. The fans make one revolution for each metre of wind that passes, and send an electric current to the brontometer, where it acts on an electro-magnet, and tends to draw this (2nd) pen towards the left; but a train of clockwork is constantly tending to draw the pen to the right, the joint result being that the pen continuously shows, not the total motion (as is the case with most anemometers), but the actual velocity almost second by second. It does this certainly with an error of less than five seconds, for the fans will stop dead in less than that time, and the clockwork train will bring the pen from indicating a velocity of 70 miles an hour to 20 miles an hour in three seconds, and down to a dead calm in seven seconds. The trace will thus resemble that of a pressure anemometer, but with a much more open scale than was ever before available.

The third pen is actuated by a handle, and can be set at zero or at 1, 2, 3, or 4 spaces from it. The author's original idea was, partly by watching a storm-rain-gauge, and partly by estimation, to decide on the intensity of the rain and to indicate that intensity by moving the pen further and further from zero as the fall becomes heavier. Experience alone will show whether that is, or is not, superior to moving it one step for each $\frac{1}{100}$th of an inch of fallen rain, which can be done by making a Crosley rain-gauge send a current into the room where the brontometer is placed, and strike a bell there. In a heavy storm there will, however, be so much for the observer to do, that very probably count would be lost. It may, therefore, be necessary to make it act automatically.

The fourth pen is actuated somewhat like a piano. On the occurrence of a flash of lightning, the observer presses a key, the pen travels slightly to the right, and flies back to zero. Referred to the automatic time-scale, this gives, to a second, the time at which the key was depressed.

The fifth pen is similar, but, being intended to record the thunder, the observer will continue to hold down the key until the roll is inaudible. The time of the departure of this pen from zero will evidently be later than that for the lightning by the time-interval due to the distance of the flash, and possibly something may be learned from the accurate record of the duration of the thunder.

The sixth pen is similar to the third, and is intended to record the time, duration, and intensity of hail.

The seventh and last pen is devoted to an automatic record of atmospheric pressure. As the rapid motion of the paper, which is indispensable for studying the details of a thunderstorm, has enlarged the time-scale more than a hundredfold, it was imperative that the barometric scale should itself be greatly enlarged. But the range of the barometer in London is more than 2½ in., and no enlargement less than ten times the natural (mercurial) scale would be of any use; hence a breadth of 25 in. of paper would be necessary, unless some mode of shifting the indication could be devised.

Several plans were tried, but finally a modification of Richard's statoscope has been adopted, which is so sensitive that it will indicate the opening or shutting of a door in any part of the house, gives a scale of 30 in. for each mercurial inch (*i.e.*, about three times that of a glycerine barometer), and yet only requires 4 in. breadth of the brontometer paper. Without entering into all the details of construction, it is desirable to explain the general principle, and its application. As it was essential that the apparatus should record accurately to 0·001 in. of mercurial barometric pressure, it was evident that friction had to be reduced to a minimum, and considerable motive power provided. This is done by placing in the base of the brontometer a galvanized iron chamber, which contains about 3½ cubic feet of air; on the upper part are a series of elastic chambers, similar to the vacuum boxes of aneroid barometers, but much larger.

When the instrument is to be put in action, these chambers are connected with the large air-chamber, and a tap is closed which shuts off communication with the external air. Any subsequent increase, or decrease, of atmospheric pressure will compress or allow to dilate the air in these chambers, and the motion of the elastic ones produces that of the recording pen.

Obviously, any large change in the temperature of the confined air would vitiate the readings; but (1) the instrument is not required to give absolute, but merely differential, values, and (2) the influence

of the changes of temperature is greatly reduced by the chamber being surrounded with 4 in. thick of non-conducting material, besides nearly 1 in. of wood outside of it. The change of temperature in a room, and during the short time that the statoscope will be worked without resetting to zero (*i.e.*, without opening the tap), has not hitherto produced any measurable effect.

The author hopes, in a subsequent paper, to have the honour of laying before the Society the results obtained from this novel apparatus.

Appendix.

List of some Papers and Memoirs on the subject of the Paper.

PLANER, J. J. "Obs. Oscillationis Mercurii in Tubo Torricelliano Erfordiæ instituta." 'Acta Acad. Moguntinæ,' 1782–3.

ROSENTHAL, G. E. ["Merkmale für das Herannahen d. Gewitter."] 'Mag. Neueste Physik,' vol. 4, Part I, 1786.

TOALDO, G. "Dei moti del Barometro nei Temporali." 'Giornale Astro-Meteorologico,' 1794.
(The note is dated April 3rd, 1795; see also his collected works—'Completa Raccolta, &c.,' Venezia, 4 vols., 8vo., 1802, vol. 3, p. 104, and vol. 4, p. 41.)

FRENZEL, F. C. ["Veränderung d. Barometerstandes bei Gewittern."] Gren's 'Neues Journal der Physik,' vol. 4 (1797), p. 250.

GRONAU, K. L. "Ueber die Gewitter in den Gegenden von Berlin." 'Schweigger's Journal,' vol. 31 (1821), p. 128.

STREHLKE, F. "Ueber d. Einfluss d. Gewitter auf den Barometerstand." 'Poggendorff's Annalen,' vol. 19 (1830), p. 148.

KAEMTZ, L. F. 'Lehrbuch der Meteorologie,' vol. 1 (1831), p. 351.

JOHNSON, M. J. "On the Detection and Measurement of Atmospheric Electricity by the Photo-barograph and Thermograph." 'British Assoc. Report,' 1855, Part II, p. 40.

—— 'Meteorological Observations made at the Radcliffe Observatory, Oxford,' 1857, p. 34.

GOULLON. "Élévation brusque du Baromètre pendant un Coup de Tonnerre." 'Bulletin International,' Mars 21, 1866.

WHITEHOUSE, W. "On a new Instrument for recording minute Variations of Atmospheric Pressure." 'Roy. Soc. Proc.,' vol. 19 (1871), p. 491.

ABERCROMBY, Hon. R. "On the Barometric Fluctuations in Squalls and Thunderstorms." 'Meteor. Soc. Quart. Journ.,' vol. 2 (1875), p. 450.

FINES. "Courbes du Barographe Redier à Perpignan pendant les Orages des 7 et 11 Juillet, 1877." 'Quinzaine Météorologique,' 1877, p. 66.

MASCART, E. "Sur l'Inscription des Phénomènes Météorologiques, en particulier de l'Électricité et de la Pression." 'Journal de Physique,' vol. 8 (1879), p. 329; and 'Annuaire de la Soc. Mét. de France,' vol. 28 (1879), p. 7.

SCHENZL, G. "Ungewitter vom 23 Februar, 1879." 'Zeits. d. Oesterr. Gesells. f. Meteor.,' vol. 14 (1879), p. 146.

RENOU, E. [Diagrams only.] 'Annales du Bureau Central Météor.,' 1880, Part I.

Fines. "Climatologie du Rousillon."
'Annales du Bureau Central Météor.,' 1881, Part I, B. 118.

Assmann. [Interesting reproductions of the Magdeburg barograph curves are given in the 'Jahrb. der Meteor. Beob. der Wetterwarte der Magdeburgischen Zeitung,' Jahr. 1—5, Magdeburg, 1883-87.]

Köppen. "Ueber Barometerschwankungen beim Gewitter."
'Tageblatt der 57 Naturforscherversammlung zu Magdeburg,' 1884, p. 301.

Hellmann, G. "Eine historische Bemerkung."
'Zeits. d. Oesterr. Gesells. f. Meteor.,' vol. 19 (1884), p. 43.

Bezold, W. von. "Ueber die Vertheilung des Luftdruckes und der Temperatur während grösserer Gewitter."
'Zeits. d. Oesterr. Gesells. f. Meteor.,' vol. 19 (1884), p. 281.

Schönrock, A. "Ueber Kleine unregelmässige Schwankungen des Luftdruckes nach den Aufzeichnungen des Barographen."
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The Society adjourned over Ascension Day to Thursday, May 22.

Presents, May 8, 1890.

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May 22, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A Contribution to the Etiology of Diphtheria."* By E. KLEIN, M.D., F.R.S. Received April 25, 1890.

The microbe, which was first described by Klebs (at the Wiesbaden Congress in 1883), then isolated and grown in artificial cultures by Löffler ('Mitth. aus dem K. Gesundheitsamte,' vol. 2) from human diphtheritic membrane, was shown by this observer to act virulently on various animals. The Klebs-Löffler bacillus—by which name the diphtheria microbe is known—is the one with which also Roux and Yersin ('Annales de l'Institut Pasteur,' vol. 2, 1888, No. 12) obtained positive results on guinea-pigs.

In the Reports of the Medical Officer of the Local Government Board for 1888–1889 and 1889–1890, I have shown that there occur in diphtheritic membranes two species of bacilli, very similar in morphological respects, and also in cultures on serum and on agar, but differing from one another in this, that one species, Klebs-Löffler bacillus No. 1, is not constant in diphtheritic membranes, does not grow on solid gelatine at 19—20° C., and does not act pathogenically on animals; the other species, Klebs-Löffler bacillus No. 2, is constant in diphtheritic membranes, in fact is present even in the deeper layers of the membranes in great masses and almost in pure culture, acts very virulently on animals, and grows well on gelatine at 19—20° C. Löffler, and after him other observers (Flügge, 'Die Mikroorganismen,' 1886), considered it as a character of the diphtheria bacillus that it does not grow on gelatine below 22° C., but this character, though true of the Klebs-Löffler species No. 1, does not appertain to the diphtheria bacillus species No. 2. In fact, there is no difficulty in obtaining pure cultures of this bacillus on gelatine if a particle of diphtheritic membrane be taken and well shaken in two or three successive lots of sterile salt solution, and from the last lot plate

* This research was undertaken for the Medical Department of the Local Government Board, and is communicated to the Royal Society with the permission of the Medical Officer.

cultivations on gelatine are made. In this way I have obtained the diphtheria bacillus in great numbers of colonies and in pure culture. Zarniko ('Centralbl. f. Bakteriol. u. Parasit.,' vol. 6, 1889, p. 154) and Escherich (*ibid.*, vol. 7, 1890, p. 8) both state that the diphtheria bacillus does grow on gelatine below 20° C.

This bacillus diphtheriæ acts very virulently on guinea-pigs on subcutaneous inoculation; at the seat of the injection a tumour is produced, which in its pathology and in microscopic sections completely resembles the diphtheritic tissue in man. In human diphtheria the diphtheria bacillus is present only in the diphtheritic membrane, but neither in the blood nor in the diseased viscera; the same holds good for the experimental guinea-pigs. In subcutaneous inoculation with artificial culture, though it causes in these animals acute disease and death—the lungs, intestine, and kidney are greatly congested—the diphtheria bacillus remains limited to the seat of inoculation. It was for these reasons that Löffler concluded that in diphtheria the diphtheritic membrane alone is the seat of the multiplication of the diphtheria bacillus, and that here a chemical poison is produced, which absorbed into the system causes the general diseased condition and eventually death. Roux and Yersin have then separated from artificial broth cultures the bacilli and the chemical products, and, by the injection of these latter alone into guinea-pigs, have produced a general effect. I have in this year's Report to the Medical Officer of the Local Government Board (1889–1890) shown that in these experiments of injection of cultures into guinea-pigs, an active multiplication of the diphtheria bacilli at the seat of inoculation can be demonstrated by culture experiments; from the local diphtheritic tumour and the nearest lymph glands the diphtheria bacilli can be obtained in pure culture on gelatine.

On various occasions during the last three years information has reached me by Health Officers (Dr. Downes, Mr. Shirley Murphy, Dr. Thursfield) as to a curious relation existing between a mysterious cat disease and human diphtheria in this manner, that a cat or cats were taken ill with a pulmonary disease, and while ill were nursed by children, and then these latter sickened with well-marked diphtheria. Or children were taken ill with diphtheria, and either at the same time or afterwards the cat or cats sickened. The disease in the cat was described as an acute lung trouble; the animals were quiet, did not feed, and seemed not to be able to swallow; in some cases they recovered, in others they became emaciated, while the lung trouble increased, and ultimately they died. In one instance—in the north of London, in the spring, 1889—this cat malady, occurring in a house where diphtheria soon afterwards appeared amongst the children, was of a widespread nature; a veterinary surgeon—Mr. Daniel—informed me that at that time he had several

patients amongst cats affected with the disease, consisting in an acute catarrhal affection, chiefly of the respiratory passages. He furnished me with two such animals: one that after an illness of several weeks had died, another that was sent to me in a highly emaciated state, affected with severe broncho-pneumonia; this animal was paralysed on the hind limbs. In both instances the *post-mortem* examination showed severe lung disease, broncho-pneumonia, and large white kidneys due to fatty degeneration of the entire cortex. A similar condition is met with in the human subject in diphtheria. Further, I received from Dr. Thursfield, of Shrewsbury, the body of a cat that had died after a few days' illness from pneumonia in a house in which children were ill with diphtheria; another cat in the same house that became next ill with the same lung trouble also succumbed. The *post-mortem* examination of the animal that I received showed severe broncho-pneumonia and large white kidneys, the entire cortex being in a state of fatty degeneration.

Subcutaneous inoculations of cats were carried out with particles of fresh human diphtheritic membranes and with cultures of the diphtheria bacillus ('Report of the Medical Officer of the Local Government Board,' 1889–1890); hereby a local diphtheritic tumour was produced at the seat of inoculation, and a general visceral disease; in the cases in which death followed after a few days the lungs were found much congested; when death followed after one or more weeks, the lungs showed broncho-pneumonia and the kidneys were enlarged and white, the cortex being in a state of fatty degeneration; if the disease in the animals lasted beyond five to seven days, both kidneys were found uniformly white in the cortex; if of shorter duration, the fatty degeneration was sometimes only in patches. Although in these experiments the bacillus diphtheriæ was recoverable by cultivation from the diphtheritic tumour at the seat of inoculation, there were no bacilli found in the lungs, heart's blood, or kidney, and the conclusion is justified that, just as in the human diphtheria and in the diphtheria produced by subcutaneous inoculation in the guinea-pig, so also in these experimental cats the visceral disease must be a result of the action of a chemical poison produced by the diphtheria bacillus at the seat of inoculation.

From this it is seen that the similarity between the artificial disease and the natural disease in the cat is very great, and the question that presents itself is, In what manner does the animal receive or give the diphtheritic contagium in the natural disease? The natural disease in the cat is in its symptoms and pathology a lung disease, and it is reasonable to suppose from analogy that the lung is the organ in which the diphtheritic process in the cat has its seat. The microscopic examination of the diseased lung of cats that died from the natural disease bears this out, the membrane lining the bronchi in the

diseased portions of the lobules presenting appearances which in microscopic character coincide with the appearances in the mucous membrane of the human fauces, pharynx, or larynx in diphtheria. But the correctness of the above supposition, that diphtheria has its seat in the lung of the cat naturally diseased, was proved by direct experiment. Broth culture of the bacillus diphtheriæ was introduced into the cavity of the normal trachea without injuring the mucous membrane. The animals became ill with acute pneumonia, and on *post-mortem*, two to seven days after, there was found extensive pneumonia, and fatty degeneration of the kidney. The bronchi, infundibula, and air cells of the inflamed lobules were found occluded by, and filled with, exudation which under the microscope bears a striking resemblance to human diphtheritic membranes, and in the muco-purulent exudation in the large bronchi and trachea the diphtheria bacilli were present in large numbers.

During the last ten or twelve years certain epidemics of diphtheria have occurred which were traced to milk, but the manner in which that milk had become contaminated with the diphtheritic virus could not be demonstrated, although the evidence as to the milk not having been directly polluted from a human diphtheria case was very strong. The epidemic of diphtheria that prevailed in the north of London, in 1878, investigated by Mr. Power for the Local Government Board, then the epidemic that occurred in October, 1886, at York Town and Camberley, the epidemic in Enfield, at the beginning of 1888, and in Barking, towards the autumn of 1888, were epidemics of this character. Mr. Power, in his Report to the Local Government Board on the York Town and Camberley outbreak, states (page 13) that a veterinary surgeon had certified that the cows from which the infected milk was derived were all in good health, but that two of the cows showed "chaps" on their teats, and he adds that even two or three weeks after the epidemic had come to an end—the use of milk having been in the meanwhile discontinued—he saw at the farm one cow which had suffered from chapped teats. At Enfield a veterinary inspector had also certified that the cows were in good health; but at Barking the veterinary inspector found sores and crusts on the udder and teats of the cows.

I have made experiments on milch cows with the diphtheria bacillus, which appear to me to throw a good deal of light on the above outbreaks of diphtheria.

Two milch cows* were inoculated with a broth culture of the diphtheria bacillus derived from human diphtheria. In each case a Pravaz syringeful was injected into the subcutaneous and muscular

* The cows had been kept under observation previous to the experiment for ten days and were in all respects perfectly normal.

tissue of the left shoulder. On the second and third days there was already noticed a soft but tender swelling in the muscle and the subcutaneous tissue of the left shoulder; this swelling increased from day to day, and reached its maximum about the end of the week; then it gradually became smaller but firm. The temperature of both animals was raised on the second and third day, on which days they left off feeding, but after this became apparently normal. Both animals exhibited a slight cough, beginning with the eighth to tenth day, and this gradually increased. One animal left off feeding and ruminating on the twelfth day, "fell in" considerably, and died in the night from the fourteenth to fifteenth day; the other animal on the twenty-third to twenty-fourth left off taking food, "fell in" very much, and was very ill; it was killed on the twenty-fifth day.

In both animals, beginning with the fifth day, there appeared on the skin of the udder, less on the teats, red raised papules, which in a day changed into vesicles, surrounded by a rim of injected skin. The contents of the vesicles were a clear lymph; the skin underneath was much indurated and felt like a nodule; next day the contents of the vesicle had become purulent, *i.e.*, the vesicle had changed into a pustule; in another day the pustule dried into a brownish-black crust, with a sore underneath; this crust became thicker and larger for a couple of days, then became loose, and soon fell off, a dry healing sore remaining underneath. The whole period of the eruption of papules, leading to vesicles, then to pustules, and then to black crusts which, when falling off, left a healing dry sore behind, occupied from five to seven days. The eruption did not appear in one crop: new papules and vesicles came up on the udder of one cow almost daily between the fifth and eleventh day after inoculation, in the other cow between the sixth and tenth day; the total number of vesicles in the former cow amounted to about 24 on the udder, four on the teats; in the latter they were all on the udder and amounted to eight in all. The size of the vesicles and pustules differed: some were not more than $\frac{1}{8}$th of an inch, others larger, up to $\frac{1}{2}$—$\frac{3}{4}$ of an inch in diameter; they had all a rounded outline, some showed a dark centre. From one of the above cows on the fifth day milk was received from a healthy teat, having previously thoroughly disinfected the outside of the teat and the milker's hand; from this milk cultivations were made, and it was found that 32 colonies of the diphtheria bacillus without any contamination were obtained from 1 cubic centimetre of the milk.

Unlike in man, in the guinea-pig and in the cat the diphtheria bacillus passed from the seat of inoculation into the system of the cow; this was proved by the demonstration of the diphtheria bacillus in the milk. But also in the eruption on the udder, the presence of the diphtheria bacillus was demonstrated by microscopic

specimens and particularly by experiment. With matter taken from the eruption—vesicles and pustules—of the udder, two calves were inoculated into the skin of the groin; here the same eruption made its appearance: red papules, rapidly becoming vesicular, then pustular, and then becoming covered with brown-black crusts, which two or three days after became loose and left a dry healing sore behind. More than that, the calves that showed this eruption after inoculation became affected with severe broncho-pneumonia and with fatty degeneration of the cortex of the kidney. In the two cows above mentioned, on *post-mortem* examination, both lungs were found highly congested, œdematous, some lobules almost solid with broncho-pneumonia in the upper lobes and the upper portion of the middle or lower lobe respectively; the pleural lymphatics were filled with serum and blood. Hæmorrhages in the pericardium and lymph glands, and necrotic patches were present in the liver. At the seat of inoculation there was in both cases a firm tumour consisting in necrotic diphtheritic change of the muscular and subcutaneous tissue. In this diphtheritic tumour continuous masses of the diphtheria bacillus were present; their gradual growth into and destruction of the muscular fibres could be traced very clearly.

It appears then from these observations that a definite disease can be produced in the cow by the diphtheria bacillus, consisting of a diphtheritic tumour at the seat of inoculation with copious multiplication of the diphtheria bacillus, a severe pneumonia, and necrotic change in the liver; the contagious nature of the vesicular eruption on the udder and excretion of the diphtheria bacillus in the milk prove that in the cow the bacillus is absorbed as such into the system.

From the diphtheritic tumour by cultivation, pure cultures of the diphtheria bacillus were obtained; a small part removed from the tumour with the point of a platinum wire, and rubbed over the surface of nutrient gelatine or nutrient agar, yielded innumerable colonies of the diphtheria bacillus without any contamination. In cultural characters in plate, streak, and stab cultures, and in cover-glass specimens of such cultures, this cow diphtheria bacillus coincided completely with the human diphtheria bacillus, but in sections through the diphtheritic tumour of the cow a remarkable difference was noticed between it and the bacillus from the cultures; inasmuch as in the tissue of the tumour the masses of the microbe, both in the necrotic parts, as also where growing into and destroying the muscular fibres, were made up of filaments and granular threads. But that it was really the diphtheria bacillus was proved by culture experiments and by cover-glass specimens. In the latter, the transitional forms between typical diphtheria bacillus and long filaments with terminal knob-like swellings, with spherical or oblong granules

interspersed here and there in the threads, could be easily ascertained. In the large number of cultivations that were made of the fresh tumour in both cows, the colonies obtained were all of one and the same kind, viz., those of the diphtheria bacillus; no contamination was present in any of the cultivations.

Appendix. May 20.

Since the above was sent in, the following instructive observations were made with regard to diphtheria in the cat and cow:—

At the beginning of the month of April two cats died at the Brown Institution which had been ill for several days previously. Their illness bore a remarkable similarity with the illness mentioned in the preceding pages as the natural (diphtheritic) disease of the cat, the most prominent symptom being an acute catarrhal affection of the respiratory tract; the animals became much emaciated and died. These two cats—which I will call Nos. 1 and 2—had been quite well previously and were kept in cages in a special shed, in which normal cats are generally kept and used for laboratory purposes. Now, after the above two cats, all cats which were put into this shed became affected in the same way: running of nose and eye, injected conjunctiva with muco-purulent discharge from the eye: coughing and more or less severe bronchial catarrh; the animals were quiet and did not feed. Between the beginning of April and the beginning of May fourteen cats became so affected, some more intensely than others; of these several apparently recovered after an illness of about one week to a fortnight, while five became greatly emaciated, very weak, and the lung trouble having greatly increased, they died, the illness lasting two to three weeks. In all five animals the lungs showed distinct signs of lobular pneumonia. In one cat (which I will call No. 3) there was present in the lower part of the larynx and the upper part of the trachea a whitish false membrane indistinguishable from human diphtheria membrane; sections through these parts conclusively prove this. In a second cat (No. 4) the trachea and bronchi contained a thick layer of fibrinous and purulent matter; in the other three animals the bronchi and infundibula contained purulent fibrinous exudation, but the trachea did not show any naked-eye change. In all five animals both kidneys were found conspicuously enlarged and white, the entire cortex being in a state of fatty degeneration. It is clear from this that the disease in these animals was the same disease as was mentioned above as the natural as well as the artificially induced diphtheria. Further confirmation was obtained by microscopic examination of the diphtheritic larynx and trachea of cat 3. On sections made through the affected portion of the larynx and trachea the entire mucous membrane was found con-

verted into a swollen infiltrated tissue undergoing necrosis; in this tissue the typical diphtheria bacilli could be seen in large and small nests and groups in the superficial layers, and extending from them into the deeper portions of the necrotic membrane. Also in cat 4 cover-glass specimens of the tracheal and bronchial exudation showed the presence of the typical diphtheria bacilli. Now, the above epidemic started with the disease of the two cats, 1 and 2, about the end of March, and the question arises: how did the disease originate in these two animals? No cats had been ill in this shed, and the two cats were normal when some weeks previously they were received at the Brown Institution. But during the latter half of March I had two milch cows in the stables of the Brown Institution ill with diphtheria induced by inoculation with the bacillus from human diphtheria, in fact the two cows described on a former page. They were inoculated on March 17, and, as was mentioned on a former page, showed the peculiar eruption on the udder between the 5th and 11th day; in one animal on the 5th day, *i.e.*, March 21, the diphtheria bacillus was demonstrated in the milk drawn from the udder. As soon as the eruption on the udder and the pulmonary affection in the cows were noticed strict orders were given to the attendant that the milk of both cows was to be thrown away. This order was not obeyed, since part of the milk was given to the two cats above mentioned, and these two animals became affected during the last week of March. I ought to mention, however, that, though the time at which these cats became affected is in perfect harmony with the suggestion that the consumption of the above milk of the affected cow had been the cause of their illness, the man who attended to the cows was also attending to the cats. But in view of the fact that this person was free from diphtheria, the possibility of having conveyed the disease from the cow to the cat is out of the question, particularly if we remember that milk containing the diphtheria bacilli had been actually given to the cats.

II. "The Chemical Products of the Growth of *Bacillus anthracis* and their Physiological Action." By SIDNEY MARTIN, M.D., Pathologist to the Middlesex Hospital. Communicated by Dr. KLEIN, F.R.S. Received May 7, 1890.

The work here recorded was done for the Medical Officer of the Local Government Board, whose permission I have for publishing this abstract of it.

The research was commenced in May, 1889. The bacilli were grown in a solution of pure alkali-albumin (made from serum-

proteids) and of mineral salts of the composition of the salts of the serum.

The cultivation of the bacilli was continued for 10—15 days, and the organisms removed by filtering through Chamberland's filter. The filtrate contained the products of the bacterial growth, viz.:—

1. *Proto-albumose* and *deutero-albumose*, and a *trace of peptone*: all with the same chemical reactions as the similar bodies formed in peptic digestion.

2. *An alkaloid.*

3. Small quantities of *leucin* and *tyrosin*.

The chief characteristic of the anthrax proto- and deutero-albumose is their strong alkalinity in solution—an alkalinity not removed by absolute alcohol, by benzene, chloroform, or ether, nor by prolonged dialysis. Acid-alcohol dissolves from the alkaline albumoses a trace of a poisonous body, but this is not in proportion to the toxicity of the albumoses. The albumoses are precipitated in an alkaline condition by saturation with NaCl (proto-albumose) or $(NH_4)_2SO_4$. The alkaloid is soluble in absolute alcohol, amyl alcohol, and in water; insoluble in benzene, chloroform, and ether. It is strongly alkaline in solution, and a powerful base, readily forming salts with acids. The sulphate crystallises in small needles or prisms; the oxalate in long, branching needles or flat plates. From the salts the alkaloid is easily regained. In solution, the alkaloid is precipitated by phospho-tungstic, phospho-molybdic, and phospho-antimonic acids and platinic chloride, but not by potassio-mercuric iodide.* It is slightly volatile, and, when kept exposed to the air, it becomes acid, and loses, to a great extent, its poisonous properties.

Physiological Action.

1. The mixture of anthrax proto- and deutero-albumose is poisonous. In small doses it produces in mice a local subcutaneous œdema, with some sluggishness, ending in recovery. Larger doses produce a greater œdema with more signs of illness, sluggishness leading to prolonged stupor, coma, and death in twenty-four hours or longer. A fatal dose for a mouse of 22 grams weight is 0·3 gram (subcutaneously injected). In some cases the spleen is enlarged: no organisms being present, as shown by gelatine tube cultivations. Boiling for a short time diminishes the toxicity of the proteid, but does not completely destroy it, and death may result from the boiled albumoses.

2. The *anthrax alkaloid* produces symptoms and lesions similar to

* With Millon's reagent, a precipitate is formed which becomes red on heating. This is the same reaction as that given by most proteids, and shows that the base is probably an amido-compound.—May 17, 1890.

the albumoses, but much more rapidly and severely. The animal becomes ill directly after the injection, gradually becomes more and more sluggish, and dies in coma, or, if a non-lethal dose be given, it recovers from the state of stupor gradually. After death enormous local subcutaneous œdema is found, with congestion and sometimes thrombosis of the small veins. Peritoneal effusion is occasionally present, and the spleen is usually enlarged, dark, and congested, or simply congested without being greatly enlarged. The fatal dose for a mouse weighing 22 grams is between 0·1 and 0·15 gram, death occurring in two to three hours.

The anthrax bacillus in digesting the alkali-albumin forms (1) proto-albumose, (2) deutero-albumose, (3) an alkaloid. The alkalinity of the albumoses may explain their toxic properties, being due to the fact that the alkaloid is in a "nascent" condition in the albumose molecule. The bacillus forms the alkaloid from the albumose, and it is possible that the living tissues have a similar action when the albumose is introduced into a living animal.

III. "On the Development of the Atrial Chamber of *Amphioxus*." By ARTHUR WILLEY, Student of University College, London. Communicated by Professor RAY LANKESTER, F.R.S. Received May 5, 1890.

Preface.

Last year, through the kindness of Professor Lankester, I had the opportunity of spending several months—May to August—in Sicily, collecting the embryos and larvæ of *Amphioxus*.

Since then I have been working continuously on the material I obtained in the laboratory of University College, under the direction of Professor Lankester. The period of the development, to which Professor Lankester determined first of all to give attention, was that before which Hatschek's well-known work stops short. He proposed that I should cut sections, so as to ascertain the mode in which the atrial chamber takes its origin and the subsequent history of the gill-slits, viz., as to how the slits on the left side of the pharynx originate. The relation of the larval to the adult mouth and the details of the curious process of movement of the mouth from a unilateral to a median position were included in the scope of our enquiries.

Professor Lankester received a grant from the Government Grant Committee in aid of the present investigation, and it is therefore necessary to state that he has constantly supervised my work, and allows

me to publish in my own name a summary of the results which I have obtained under his guidance.

Amphioxus occurs in great numbers in a comparatively small lake, or *pantano*, which is situated behind, and separated from the sea by, the village of Faro, near Messina. It is connected with the Straits of Messina by a narrow canal, some two or three hundred yards in length.

The bottom of the *pantano*, in contrast to that of the Straits, consists of foul mud; and it may be mentioned in this connexion, as I was informed by Professor Kleinenberg, that *Amphioxus* is only occasionally met with in the Straits, and is entirely absent from another larger *pantano* which lies behind the neighbouring village of Ganzirri, and is joined by a short canal to the one at Faro.

The embryos float on the surface, and are to be had by dredging on the surface at sunrise, but the readiest method of obtaining them in quantity is to take the adults in glasses and allow them to spawn there, if they will. Spawning takes place about an hour after sun-down.

The ova, if fertilised, must be very carefully distributed among several glasses containing clean, but unfiltered, water from the *pantano*. If the water is filtered, or if sea water is employed, or if too many ova are placed in one glass, they will certainly either die or develop abnormally.

The first outward and visible sign of fertilisation is the separation from the egg-cell of the yolk-membrane (*Dottermembran*).

Most, if not all, of the ova that I obtained were discharged through the atriopore.

If Kowalewsky* had not seen them issuing from the mouth, it would not have been easy to understand how they could pass into the pharynx in opposition to the constant outflow of water between the gill-bars.

Segmentation always commences at dusk—between the hours of seven and eight—and goes on very rapidly through the night.

The early stages have been so fully described by Hatschek† that I will only refer to them in the briefest manner.

At 8 P.M. segmentation commences; at 11 P.M. invagination commences; at 1 A.M. the gastrula is complete; at 3 A.M. the gastrula begins to revolve by cilia within the yolk-membrane; and at 5 A.M. two pairs of myocœlomic pouches have been formed, and the embryo ruptures the egg-membrane and becomes free-swimming.

During the first day the embryo grows in length and adds several pairs of somites. By about eight o'clock on the second morning, that

* "Entwick. des Amph. lanc." ('Mém. Acad. Impér. des Sciences de St. Pétersbourg,' Series VII, vol. 11, 1867.)

† Claus's 'Arbeiten,' 1881.

is, thirty-six hours after the commencement of segmentation, the embryo has acquired a mouth on the left side of the body, and a gill-slit, which arises at first in the median ventral line, and subsequently comes to lie on the right side of the body.

The anus is formed soon after the appearance of the mouth and first gill-slit.

The embryonic period is now at an end, and the larval period begins. As Hatschek states, the only way of obtaining the larval stages is by pelagic fishing. This consists in dredging at depths varying from fifteen to twenty fathoms. At this depth the *Amphioxus* larvæ float in the midst of countless thousands of *Sagitta* larvæ.

A long, but not yet clearly ascertained interval (probably about a fortnight) elapses between the formation of the first and second gill-clefts.

In the period during which it is free-swimming the larva acquires from twelve to fifteen consecutive unpaired gill-slits, each one arising in the mid-ventral line, and then growing in such a manner as to lie on the right side of the body. This applies to the anterior two-thirds of the pharynx, but I am not quite clear yet as to whether the last two or three median slits ever move up to the right side. Meanwhile, longitudinal ridges which are subsequently concerned in the formation of the atrium have appeared (see fig. 6).

At the time of the completion of the atrium, which occurs at the close of the larval period, some remarkable changes in the relative position of parts of the body in the anterior region take place, by which the mouth becomes median, and the gill-slits are arranged in two series, a right and a left. The larva emerges from this critical phase in its development as a symmetrical animal, but the details of the process of symmetrisation—the strongly-marked character of which justifies the use of an otherwise undesirable term—are still rather obscure. The larva, now really a young *Amphioxus*, with atrium and paired gill-slits, ceases to lead a pelagic life, and takes to the sand, where it passes the rest of its life.

Spawning occurs at least from April to September inclusive. The best month, however, in which to obtain the embryos is June, while all the larval stages, up to the passage into the adult form, are to be found during July and August.

Previous View as to the Formation of the Atrium.

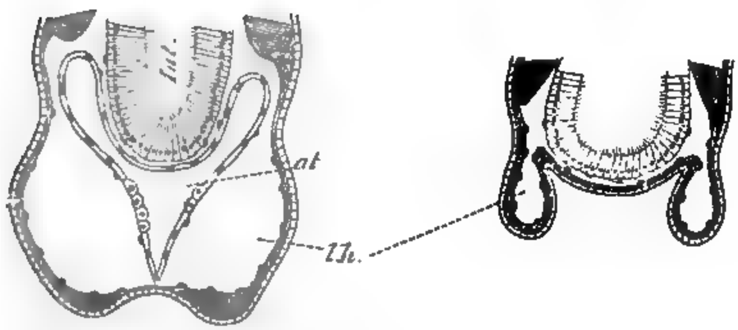
The hitherto accepted method of formation of the atrial chamber of *Amphioxus* is that described by Kowalewsky,* and more fully by Rolph.†

* 'Archiv für Mikrosk. Anat.,' vol. 13, 1877.

† 'Morphol. Jahrbuch,' vol. 2, 1876.

Kowalewsky says that after a certain number of gill-slits have been formed two longitudinal folds appear on opposite sides of the body, which grow round and meet, and finally fuse together in the median ventral line, leaving a wide aperture at one end—the atriopore. His figures, two of which are here reproduced (figs. 1 and 2),

Fig. 1. Fig. 2.

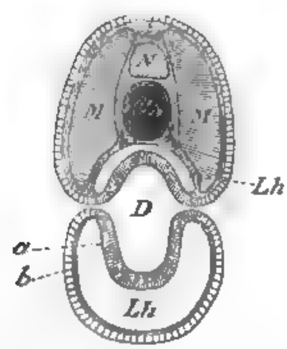


Copies of Kowalewsky's figures of transverse sections through a larva of *Amphioxus* with fully-formed atrium. Fig. 1 represents a section taken between pharynx and atriopore; and fig. 2, one taken just behind the atriopore of the same larva.

Int. Intestine. *at.* Atrium. *Lh.* Cœlom.

bear this description out, more or less, while Rolph's schematic figures bear it out entirely. The latter are reproduced in figs. 3, 4, and 5.

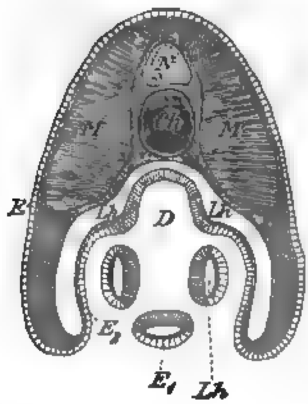
Fig. 3.



Copy of Rolph's theoretical section through the pharyngeal region of a larva, before the commencement of the so-called epipleural folds.

N. Nerve-cord. *Ch.* Notochord.
M. Muscles. *Lh.* Cœlom.
D. Intestine. *a.* Intestinal epithelium.
b. Epidermis.

FIG. 4.

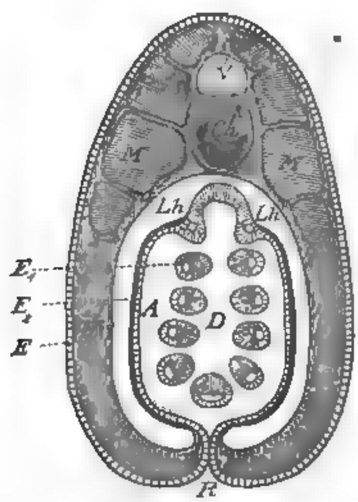


Copy from Rolph of a similar section through an older larva, showing the commencing longitudinal downgrowths.

E. Epidermis.
E_1. Inner epithelium of the (future) atrial cavity.
E_2. Outer epithelium of same.
U. Subcutaneous tissue.

Other letters as in fig. 3.

FIG. 5.



Copy from Rolph of a similar section showing the meeting together of the "*epipleura*" in the ventral middle line.

A. Atrium. *R.* Raphe.

Other letters as in figs. 3 and 4.

The most serious error in Kowalewsky's view lies in the fact that he makes the space in the lateral outgrowths continuous with the body-cavity, and consequently calls it "Leibeshöhle," or cœlom.

This space, as we shall see, does not belong to the true cœlom, and is not traceable as a space to the original myocœlomic pouches, but arises apparently as an inter-cellular space in the midst of the connective tissue—in fact, it would seem to belong to that category of spaces to which the term "pseudocœl" has been applied. In this respect it stands in contrast to the spaces in connexion with the dorsal and ventral fins, which have been shown by Hatschek to be derived directly from the myocœlomic pouches.

Rolph's figures (figs. 3, 4, 5) do not profess to be more than diagrams. They show the epipleur originating as a depending ridge on each side of the pharynx (fig. 4). Into this ridge the cœlom is extended. The epipleura meet finally in the middle line below the pharynx according to this theory (fig. 5). It is no doubt true that the scheme of growth thus sketched by Rolph, and based upon Kowalewsky's erroneous figures, would account satisfactorily for the condition of the atrial chamber and its epipleural walls as observed in the adult. It also gives a basis for the suggestion made by Kowalewsky that the epipleura are comparable to the opercula of Teleostean fish.

I now propose to show that this view is based on erroneous observation.

Formation of the Atrial Chamber as observed by me.

The first indication of the commencing formation of the atrial chamber is to be found in larvæ with nine or ten gill-slits on the right side. Behind the region of the pharynx we find that the mid-line of the body has become marked with a narrow groove, so that in section it is bifid (fig. 6). The short upstanding ridges which limit the groove are the metapleura of the adult. Though at first solid, the connective tissue within the ridge soon becomes hollowed, and forms a lymph space which never has any obvious connexion with the cœlom. These ridges can be traced from about the middle of the larva's body forward towards the pharyngeal region, where they diverge considerably from one another. That belonging to the animal's left side keeps a more or less median position, and can be traced (though but small in elevation) when twelve gill-slits are present as a ridge situated at the lower or ventral margin of the gill-slits and dying out in the anterior region of the pharynx. The right-hand ridge, or metapleur, takes a course to the right of the gill-slits (which, it will be remembered, are on the right side of the body), and overhangs the upper limit of the slits to a small extent. It dies out in front of the first gill-slit, where it bends towards the middle line.

Fig. 6. Fig. 7.

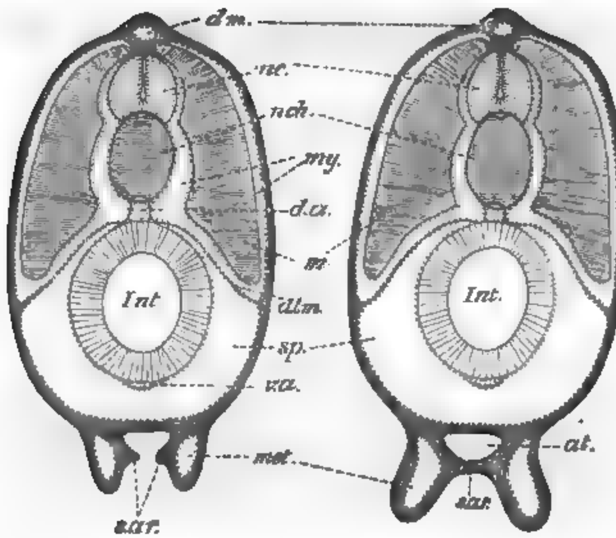


Fig. 6.—Transverse section through a larva with eleven or twelve unpaired gill-slits and with lateral mouth, showing the minute sub-atrial ridges.

d.m. Dorsal division of myocœl in which the fin-ray will lie when it is developed.
nc. Nerve-cord.
nch. Notochord.
m. Muscle-plate.
my. Cavity of myocœl.
d.a. Dorsal aorta.
Int. Intestine.
d.l.m. Double-layered membrane separating the myocœl from the splanchnocœl.
sp. Primitive splanchnocœl.
v.a. Ventral vessel.
met. Metapleur.
s.a.r. Sub-atrial ridges.

Fig. 7.—Transverse section through a slightly older larva. The sub-atrial ridges (*s.a.r.*) have fused for a short distance between atriopore and pharynx; but in the pharyngeal region the atrium is unclosed, and consequently the gill-slits still open directly to the exterior.

at. Atrium.

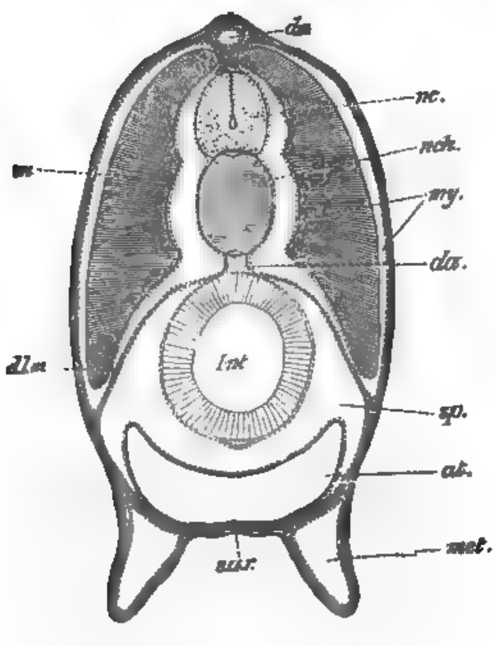
The atrium is formed by a small horizontal growth (*s.a.r.* in fig. 6), which starts from the inner face of each metapleur and floors in the deeper half of the groove or area between the two metapleura (fig. 7, *at.*).

These horizontal growths may be called the sub-atrial folds.

They are at first extremely small, and the atrial space floored in is a mere canal. Later the width of the atrial space increases greatly, and the sub-atrial folds consequently widen also, becoming that pleated expansible floor of the atrial chamber, with its transverse muscular layer, which all observers of *Amphioxus* know so well (fig. 9, *s.a.r.*).

The atrial groove becomes floored in first in the region of the atriopore. The growth of the sub-atrial folds extends gradually forwards, and the closure proceeds along one side (the right) of the pharynx. The whole atrium thus formed is a very small tube-like space. The closure by means of the small horizontal sub-atrial outgrowths in the region of the large gill-slits is somewhat difficult to explain. The small left metapleur actually moves in course of growth from the mid-line, and rises on to the right side somewhat. At the same time the much larger right metapleur is deepened, and overhangs the slits. Then the little horizontal junction is effected, and we get actually a nearly tubular atrium receiving the openings of successive gill slits. With subsequent growth the narrow atrial

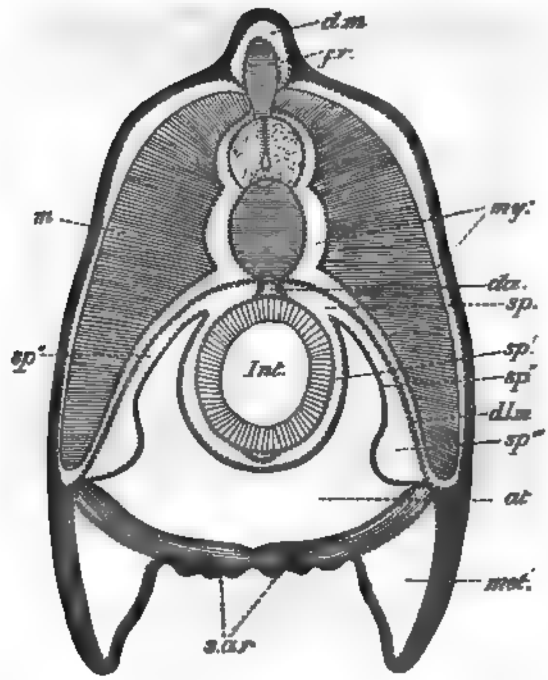
FIG. 8.



Transverse section through an advanced larva with fully-closed atrium. The latter has begun to encroach on the cœlom (*sp*).

Letters as in figs. 6 and 7.

FIG. 9.



Transverse section through an adult *Amphioxus*. The atrium has grown up so as to divide the primitive splanchnocœl into two portions—an inner or splanchnic, and an outer or parietal, portion (*sp'*. and *sp.''*).

sp. The portion of the primitive splanchnocœl which is not so affected by the atrium, and which persists as the dorsal cœlom.

sp''. The parietal part of the splanchnocœl.

sp'''. Its expansion as perigonadial cœlom.

f.r. Fin-ray.

Other letters as in figs. 6 and 7.

(The above figures 6—9 are all somewhat diagrammatic.)

tube widens and pushes itself right and left, so as to encroach on the space hitherto occupied by the cœlom, and finally it extends so far dorsalwards as nearly to surround the alimentary canal (see figs. 8 and 9).

The evidence of this history, in the form of careful drawings of consecutive sections from snout to atriopore, at various stages in the closure of the atrium, I purpose to publish shortly in the 'Quarterly Journal of Microscopical Science.' For the present I am anxious to point out, firstly, that this mode of formation of the atrium as a narrow groove, which closes and sinks (as it were) into the body of the *Amphioxus*,

is really different in important respects from the enclosure of a space by downgrowth of large folds, though ultimately no doubt the two contrasted modes of formation come to the same thing so far as the more obvious morphological relations are concerned. The mode of formation which really occurs in *Amphioxus* is readily harmonised with the existence of the post-atrioporal extension of the atrium which gradually tapers to a fine cæcal canal. It also gives us an essentially different view of the region called "epipleur" by Lankester, and generally so designated, from that which Rolph's theory necessitated. That portion of the epipleur into which the myotomes of the body-wall extend is seen now to be no downgrowth, no extension or fold. It is the original unchanged body-wall which bounds the sides of the animal's body in front of the atriopore, just as much as it does behind. The only *new* growth in the atrial region which takes part in the limitation of the surface is the sub-atrial growth formed by the two little horizontal folds which floor in the atrium when it is a mere canal. These in the adult are represented by the limited region of longitudinally pleated ventral wall between the two metapleura.

Lastly, the formation of the atrium as a narrow groove which closes, sinks into, and expands within the body of *Amphioxus*, is much more readily comparable to what is known of the formation of the atrial chamber in the Ascidians than is the Kowalewsky-Rolph scheme. In the Ascidian a pair of in-pushings are formed, each with a circular orifice of invagination; they expand within the body, fuse with one another to form one cavity, and one of the circular orifices disappears. In *Amphioxus* we have a single in-pushing with a longitudinal orifice of invagination, which closes as the invagination forms, excepting at its hindermost border, and then expands to a greatly increased volume.

The comparison of the so-called epipleura of *Amphioxus* with the opercula of Fishes has only a remote morphological basis, and probably no genetic relationship exists between these two structures.

IV. "On a Method of determining the Value of Rapid Variations of a Difference of Potential by means of the Capillary Electrometer." By GEORGE J. BURCH, B.A. Communicated by Professor BURDON SANDERSON, F.R.S. Received April 25, 1890.

In 1882 a paper by Professor Burdon Sanderson* appeared in the 'Biologisches Centralblatt,' in which an account was given of the

* Burdon Sanderson, "Die elektrischen Erscheinungen am Dionaeablatt," 'Biologisches Centralblatt,' 15 Oct., 1882.

employment of photography for the production by means of the capillary electrometer of records of the electrical phenomena accompanying the excitation of the leaf of *Dionæa*. Other physiologists have availed themselves of this method for the purpose of determining the times at which the electrical changes in living tissues begin and end, but no complete investigation has been made of the relation between the curves obtained and the amount of variation of the difference of potential of which they are the expression.

A series of experiments which I undertook two years ago, at the suggestion of Professor Burdon Sanderson, for the purpose just indicated, has led to the discovery of a method by which it is possible to determine from the photographed curve the difference of potential corresponding to any part of its course.

I propose in this preliminary note to state briefly the experimental results upon which the method is based, to describe the application of it in its simplest form to the analysis of a photographed curve, and, finally, to indicate the corrections which have to be applied to the approximation thus obtained.

I. *The Normal Curve.*

The magnified image of the capillary, pointing vertically downwards, is projected through a slit upon a sensitive plate travelling horizontally at a uniform rate. During the passage of the plate behind the slit a sudden permanent difference of potential of known value is introduced between the terminals of the electrometer, causing the meniscus to move from its original position of rest to another level, the movement being recorded on the plate by the curved outline of the shaded portion. This will be referred to as the "normal curve," and the movement as a "normal excursion."

(1.) By direct photographic evidence I have proved that there is no measurable delay between the communication of a difference of potential and the commencement of the movement of the meniscus.

(2.) *Under ordinary circumstances* the meniscus ceases to move the instant the source of electromotive force is withdrawn. [See also (7)].

(3.) The normal curve is of the same form and dimensions *so far as it extends*, no matter what may have been the difference of potential which produced it. The accompanying diagram illustrates a normal curve taken with a large resistance, and low down in the capillary, so that variations of resistance during the excursion [see (7)] may be neglected. The entire curve corresponds to a difference of potential of 0·01 volt. The portion above the line through 5 is the entire normal curve for 0·005 volt, and the portion above P is the curve for 0·00195 volt.

Oy = axis of y, graduated in $\frac{1}{1000}$ of a volt.
Ot = axis of t, and asymptote of the curve.
PN and P_1N_1 = ordinates at the points P and P_1 respectively.
PT and P_1T_1 = tangents.
NT and N_1T_1 = subtangents.
[$NT = N_1T_1$.]

(4.) The time required by the meniscus to traverse half the distance through which the sudden introduction of a permanent difference of potential would cause it to move, is, within wide limits, independent of the amount of that difference. This time may be conveniently referred to as the "time of half-charge." It is one of the constants of an instrument, and is affected only [with the exceptions indicated in (7)] by the external resistance of the circuit.

(5.) The above conditions are fulfilled if the equation to the normal curve is of the form $\log \frac{y}{a} = ct$, where y is the vertical distance of a point P upon the curve from its asymptote (*i.e.*, the level at which the meniscus finally comes to rest) and t is the horizontal distance of P from a point O upon the asymptote, which is taken as the origin of coordinates. There is a well-known characteristic of all curves having this equation, by which they can be easily recognised, namely, that the subtangent NT, or intercept upon the asymptote between the tangent PT of any point P upon the curve and its ordinate PN, is of constant length. I have accordingly measured the length of the subtangent at various points upon a number of normal curves, and find it to be constant for each electrometer, except so far as it is altered by the total resistance of the circuit and the variations of resistance, &c., which will be referred to in (7). The normal curve is therefore approximately the logarithmic curve.

II. *Curves produced by Rapid Variations, of Unknown Amount, of the Difference of Potential between the Terminals.*

(6.) From (1), (2), and (3) it follows that in all cases the velocity with which the meniscus is moving at any instant is that with which it would start if the zero line were moved to the level then occupied by the meniscus, and the difference of potential existing at the time between the terminals of the electrometer were suddenly introduced and made permanent. Thus the *total indicated difference of potential* is made up of two parts, viz., the difference represented by the distance through which the meniscus has already moved, and that indicated by the velocity with which it is still moving.

This being so, there are two ways in which the actual difference of potential at the moment corresponding to any point upon the photographed curve may be estimated. The first is to compare it with a normal curve, on which a point must be found of which the tangent coincides in direction with the tangent at the required point of the curve under examination. The vertical distance of this point upon the normal curve from its asymptote will express the remainder of the difference of potential, *provided that the resistance in circuit was in both cases the same.*

The second method depends upon the property of the logarithmic curve mentioned above. Upon an enlarged copy of the curve to be investigated, points are taken corresponding to equal intervals of time (*e.g.*, 0·001 sec.). Through each of these points a tangent and an ordinate are drawn, and produced, upwards if the curve is rising, or downwards if it is falling, until the horizontal distance between the tangent and the corresponding ordinate is equal to the length of the subtangent of the normal curve. The level at which this is the case is that to which the meniscus would have risen or fallen, had the difference of potential between the terminals at that instant been made permanent, and is in fact the position of the asymptote of the corresponding normal excursion. Consequently, a line touching the ends of all the ordinates so produced will represent the variations of difference of potential during the experiment, upon the same scale as the normal curve.

(7.) This result is only an approximation; to make it accurate three kinds of corrections have to be made, as follows:—

(*a.*) *Calibration Errors.*—The capillary may not be of equal sensitiveness throughout the part employed. In this case the electrical capacity of the electrometer viewed as a condenser will also vary with the position of the meniscus in the tube, so that the correction is difficult to make, and should be avoided by selecting a suitable instrument.

(*b.*) *Overshooting.*—In instruments with small internal resistance,

and in which the friction is reduced to a minimum, the mercury column appears to acquire a certain momentum, so that the statement in (3) no longer applies, and the instrument is not dead beat in its action. It may, however, be made so by the introduction of a sufficiently large resistance.

There is also another form of overshooting, due to the elasticity of the meniscus itself, the effect of which upon the curve is distinct, and different. Both these forms of error are rare.

(*c.*) *Variations of Resistance during an Excursion.*—The principal seat of the internal resistance of the capillary electrometer being the slender column of dilute acid in the tube, the length of which varies with the movement of the meniscus, it is evident that the total resistance must vary during an excursion. The effect of this variation of resistance may be detected in excursions of considerable extent, and the amount of it measured. I have found that the error due to this cause is seldom more in practice than one per cent. The method of applying these corrections, together with a description of the apparatus employed, and a discussion of the points of theory involved, I hope to publish at no distant date.

The investigations of which the results are given above were made in the Physiological Laboratory, Oxford, the resources of which were placed at my disposal for the purpose by Professor Burdon Sanderson.

V. "A Bacteria-killing Globulin." By E. H. HANKIN, B.A., St. John's College, Cambridge, Junior George Henry Lewes Student. Communicated by Professor ROY, F.R.S. (From the Pathological Laboratory, Cambridge.) Received May 21, 1890.

The results described in the present paper were arrived at by the author while trying to discover the nature of the substance to which the bacteria-killing powers of the blood serum were due.*

The results obtained by Nuttall,† Buchner,‡ and Nissen§ have shown that the blood serum, independently of any cellular elements, has a certain power of killing bacteria. The method used by these

* My work was aided by grants from the British Medical Association and from the John Lucas Walker Fund.

† "Experimente über die bakterienfeindlichen Einflüsse des thierischen Körpers," 'Zeitschrift für Hygiene,' vol. 4, 1888, p. 353.

‡ "Ueber die bakterientödtende Wirkung des zellenfreien Blutserums," 'Centralblatt für Bakteriologie und Parasitenkunde,' vol. 5, p. 817, and vol. 6, p. 1, 1889.

§ "Zur Kenntniss der bakterienvernichtenden Eigenschaft des Blutes," 'Zeitschrift für Hygiene,' vol. 6, 1889, p. 487.

authors, and which I have applied to this research, is to mix a small quantity of a culture with a few cubic centimeters of blood serum. A drop of this serum is immediately taken, and a gelatine plate culture made with it. Again, at intervals varying from a half to twenty-four hours, plate cultures are made containing drops of the serum. The first plate culture serves as control. The number of colonies that appear in it is observed. A progressively decreasing number of colonies will be found in the succeeding plate cultures, owing to the gradual decrease in the number of bacilli that remain in a living condition in the blood serum. A curious point about this property of blood serum is that it vanishes in the act of killing the microbes. That is to say, a given quantity of blood serum can only kill a limited number of microbes. If the number of microbes added to the serum is beyond this limit, the survivors find the blood serum to be an excellent food medium, and, after a time, begin to grow and reproduce. This fact, together with the comparatively low temperature (six hours' heating to 52° or half an hour's heating to 55°) at which the bacteria-killing power vanishes, has led some of the above-quoted authors to ascribe it to a "sort of ferment-like activity." Buchner* found that the bacteria-killing power vanished on dialysing the serum into distilled water, not, however, if it was dialysed into 75 per cent. sodium chloride solution which had been brought to the same degree of alkalinity as the serum. From this and other facts, Buchner is brought to believe that the property in question is connected with the intactness of "Nageli's hypothetical Micellæ" present in the serum, and is due to a residuum of the "life" possessed by the plasma from which it is derived. Surely, an equally obvious conclusion would be that it was connected with the presence of some unknown globulin, which, like other globulins, is only soluble in dilute salt solutions, and, therefore, would be precipitated after dialysing into water, and remain in solution on dialysing into physiological salt solution. The experiments of Lubarsch† have shown that this power practically does not exist in the living blood plasma; consequently (on the hypothesis that it is due to some specific germicide), it must be due to a substance present in blood serum absent from the plasma. Further, the above-mentioned observations suggest that it is due to a substance of the nature of a ferment.

Can it be fibrin ferment? The facts that a bacteria-killing power is possessed by peptone plasma, but not by magnesium sulphate plasma, are not in complete disaccord with this suggestion. A

* *Loc. cit.*

† "Ueber die bakterienvernichtenden Eigenschaften des Blutes und ihre Beziehungen zur Immunität," 'Centralblatt für Bakteriologie und Parasitenkunde,' vol. 6, 1889, p. 528.

watery solution of fibrin ferment loses its power of coagulating blood when heated to 75°. When, however, fibrin ferment is dissolved in serum it is destroyed, according to Halliburton,* at a temperature somewhere between 50° and 60°, which agrees with the temperature at which Buchner found the bacteria-killing power of serum to vanish. On testing the effect on anthrax bacilli of a solution of fibrin ferment by the above-described method, I occasionally found a gradual diminution in the numbers of the bacilli, but, even in the most successful experiment, it was so small as to come well within the limits of experimental error. For instance, in one experiment, the control plate showed 175 colonies. A plate culture made half-an-hour later showed 40 colonies, and the plate culture made one hour after mixture showed 111 colonies. Thus my results, though of themselves too few to decide this matter, agree with those of Buchner, who also found that fibrin ferment has no bacteria-killing power.

Can the substance in question be some other ferment, absent from the plasma and present in the serum, possibly owing to the breaking down of white blood-corpuscles?

Halliburton has recently succeeded in extracting from the leucocytes of lymphatic glands a globulin which he believes to be fibrin ferment, or else inseparably connected with it. The power of causing blood to clot, the temperature at which it loses this property, and its solubilities in different salt solutions agree perfectly with what is known of fibrin ferment. In fact, the only respect in which it differs from fibrin ferment is that it always responds to the general proteid reactions; while Lea and Green have obtained solutions, apparently of fibrin ferment, which caused blood to clot rapidly, but which responded not even to the most delicate tests for proteids.

The following experiments show that it has the power of killing anthrax bacilli, a power which, as yet, cannot be ascribed to fibrin ferment.

Methods Employed.—The lymphatic glands of an animal (cat or dog) are cut out, finely chopped up, and extracted with $\frac{1}{10}$th saturated sodium sulphate for twenty-four hours. The liquid is then filtered, and precipitated by the addition of several times its volume of alcohol. The precipitate consists chiefly of Halliburton's cell globulin-β, for the other proteids of lymph cells (cell globulin and nucleo-albumen) are only extracted in small quantities by the sodium sulphate solution.† The precipitate is kept under alcohol till required. It settles to the

* "On the Nature of Fibrin-ferment," 'Journal of Physiology,' vol. 9, 1888, p. 229; and "On the Coagulation of the Blood," Roy, Soc. Proc.,' vol. 44, 1888, p. 255.

† I learnt this in a verbal communication from Dr. Halliburton. I shall in the rest of my paper refer to Halliburton's cell globulin-β as cell globulin, or simply globulin.

bottom of the vessel, and, before using it, the superabundant spirit is poured off. The bottle is then shaken, and 5 to 10 c.c. of the turbid liquid is poured out and filtered. The precipitate remaining on the filter paper is washed with distilled water and then extracted with about 15 c.c. of a $\frac{1}{10}$th saturated sodium sulphate solution. In other cases it was at once extracted with 75 per cent. sodium chloride, or with distilled water, the adherent salt enabling the precipitated globulin to dissolve.

Always the distilled water or other solvent employed had been previously carefully sterilised. Only a very small part of the coagulated mass of proteids was redissolved, the solution containing the ferment-like cell globulin, while probably the other proteids present were permanently coagulated by the action of the alcohol. To the solution thus obtained a small quantity of anthrax culture is added. The culture is generally in bouillon and not more than twenty-four hours old. Before being added it has been violently shaken in order to separate and break up the filaments. As soon as the anthrax bacilli have been added to the globulin solution and shaken so as to distribute them uniformly through the liquid, a sample of the latter is taken, and with it a gelatine culture (which serves as control) is made. This sample is not taken out with a platinum wire, but with a capillary pipette. A kink is made in the pipette about 3 inches from the end, and while removing samples of the liquid care is taken that it should in each case be filled exactly to the kink. In this way, as the same pipette is used throughout each experiment, it is easy to abstract exactly the same quantity of liquid each time.* As soon as the pipette has been emptied into the gelatine, the end that has been used is placed in a test tube plugged with sterilised cotton-wool and containing sterilised normal salt solution. This is immediately boiled for a few minutes. Since the young culture of anthrax employed contains no spores, the pipette can be thus easily sterilised. During the experiment the globulin solution is kept at a temperature of 37°.

The following table shows some of my experiments in a tabular form:—

* A separate pipette was used to inoculate the globulin solution with anthrax.

No. of experiment.	Globulin dissolved in :—	Number of colonies in plate cultures. 1. Control made immediately after inoculation.	2. Made after ½ an hour.	3. After 1 hour.	4. After 2 hours.	5. After 3 hours.
I.	*a*. Dilute sodium sulphate (= $\frac{1}{10}$th saturated solution) ..	3962	1022	—	366	—
	b. Distilled water made faintly alkaline with KOH......	44	24	—	1	—
III.	*a*. Dilute sodium sulphate after washing with distilled water.	6256	—	1330	—	—
	b. Same solution as in *a*, but diluted with 0·75 NaCl solution.	About 7000	—	953	—	—
IV.	*a*. Globulin extracted with 0·75 NaCl solution	2079	457	—	30	0
	b. Ditto diluted 10 times.	868	494	—	535	—
V.	*a*. Globulin twice precipitated by alcohol, then dissolved in 0·75 NaCl solution.	14,798	22*	—	0	After 24 hrs. 0
	b. In dilute Na_2SO_4 solution after washing precipitated globulin with water	1174	12*	—	0	—
VII.	*a*. Globulin from spleen of dog extracted with distilled water.	2269	317			
	b. Ditto boiled for 10 minutes........................	3078	3546			
	c. Globulin from lymphatic glands of same animal	657	222			

* Colonies retarded in growth.

From this table it is obvious that a decrease took place in the number of living bacilli present in the samples that were successively taken from the globulin solution. That is to say, the bacilli were gradually killed until in some experiments (as shown by the plate cultures remaining sterile) no survivors were left. In these cases the gelatine was, after a few days, inoculated with anthrax, when it always produced a copious growth, showing that the result was in no way due to any imperfection in the gelatine employed. That the destruction was not due to sodium sulphate or other salts present is shown by Experiment VII, for here the liquid is seen to lose its bacteria-killing power by being simply boiled, a treatment capable of destroying the globulin but not the salts. In other cases the globulin solution was boiled after the plate cultures had been inoculated from it. Then it was re-inoculated with anthrax with or without addition of a small quantity of bouillon, and always produced a typical growth. In Experiment VII, after two days, the test tubes containing the solutions B and C were found to contain anthrax growths, the one in C being somewhat scanty. Solution A, however, had remained sterile, all the bacilli in it having been killed. Then, without previous boiling or any other treatment, it was inoculated with anthrax spores, and in twenty-four hours had produced a copious growth. This proves conclusively that the destruction of bacilli that had taken place was due to a similar cause to that which is operative in the analogous experiments with blood serum. For Lubarsch* has pointed out that, although the serum is capable of killing the bacilli added to it, the spores (of anthrax) not only are unharmed by it, but immediately begin to develop, producing a crop of bacilli, which seem to have acquired tolerance against the bacteria-killing power of the medium. It is interesting to notice that, as shown in Experiment VII, the cell globulin obtained from the spleen was more energetic in killing bacteria than that derived from lymphatic glands. In this experiment the spleen and the lymphatic glands were taken from the same dog, left under alcohol for the same time (twenty-four hours), and otherwise subjected to exactly similar treatment. In Experiment V the globulin was derived from the lymphatic glands and spleen of a cat, which were chopped up and extracted together. Here the bacteria-killing action was more energetic than in the previous experiments. I generally noticed that the bacteria-killing power is less, the longer the globulin is kept under alcohol, which agrees with Halliburton's assertion,† that by very prolonged action of alcohol the cell globulin is rendered permanently insoluble, as is the case with other proteids.

* *Loc. cit.*

† "On the Nature of the Fibrin Ferment," 'Proceedings of Physiological Society,' 1888 (in 'Journal of Physiology,' vol. 9).

I have as yet made no systematic observations on the degenerative appearances that anthrax bacilli may show when subjected to the action of cell globulin. On one occasion, however, I found that the bacilli, twenty-four hours after being added to a dilute cell globulin solution, had broken up into extremely short segments. Czaplewski* found a similar change to occur as a stage in the degeneration of virulent anthrax bacilli when injected into pigeons, which were naturally immune against this disease, and Gamaleia† met with a like mode of degeneration during the "vaccinal" fever which follows the inoculation of attenuated anthrax into rabbits or sheep.

My experiments on the effects of injections of cell globulin into animals before or after anthrax infection in modifying the course of the disease, though entirely of a preliminary nature, are of some interest. The cell globulin was not prepared by the above-described method, but the sodium sulphate extract was dialysed to remove the excess of the salt, and heated to 50° or 51° for half-an-hour to coagulate any traces of cell globulin-*a* or nucleo-albumen that might be present.‡ A few cubic centimeters of a solution thus prepared were injected into the lateral ear vein of a rabbit, which, either at the same time or on the day before, had been inoculated with anthrax. The results were extremely variable, and showed no relation to the quantity of cell globulin employed. In some cases no effect was produced, the animal dying in the same time as, or even before, the control.§ In other cases the animal lived two or three days longer, and showed slight diarrhœa. In such cases the spleen was nearly always greatly enlarged. The bacilli were generally in long chains (consisting of occasionally twenty or thirty joints), as is usually the case after inoculation with attenuated anthrax. Sometimes, however, they appeared to be exceptionally short. Further, a large number of phagocytes (macrophages), containing bacilli in different stages of degeneration, could be seen. In the control animal phagocytes containing bacilli could but rarely be found. Fig. 1 shows the temperature chart of three rabbits, of which two (A and B) received a succession of doses of cell globulin. The three rabbits were very young, and did not weigh more than 400 grams each; C was the control, and died in thirty-six hours. A and B showed well-marked diarrhœa forty-eight hours after inoculation, and died after ninety-

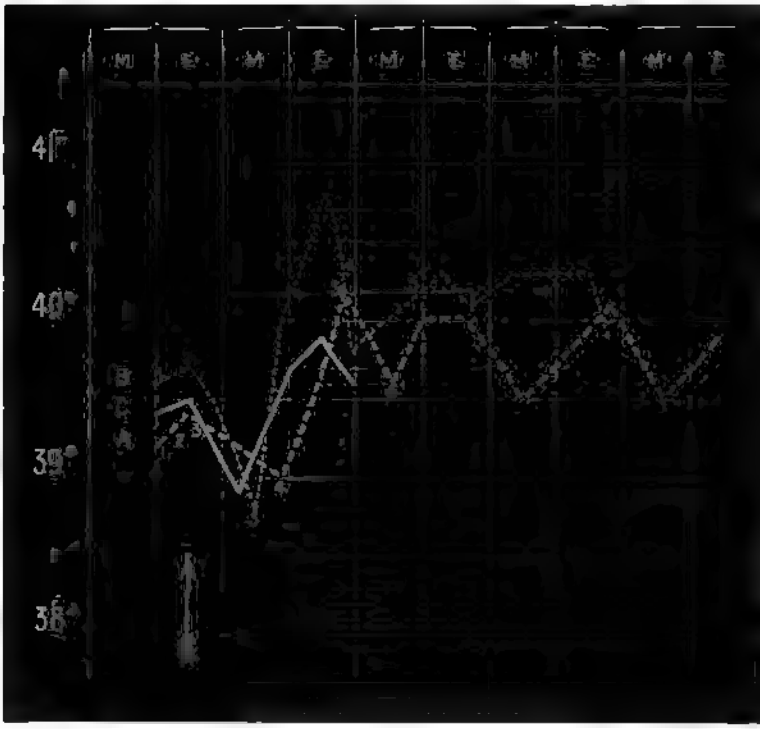
* "Ueber die Immunität der Tauben gegen Milzbrand," 'Königsberg Diss. Inaug.,' 1889.

† "Sur la Destruction des Microbes dans les Corps des Animaux fébricitants," 'Annales de l'Institut Pasteur,' 1889, p. 229.

‡ "Report of a Committee consisting of Professors Schäfer (secretary), Foster, and Lankester, and Dr. W. B. Haliiburton, appointed for the purpose of investigating the physiology of the lymphatic system" ('Brit. Assoc. Rep.,' 1888, p. 363).

§ In these experiments, the control rabbit nearly always died within thirty-six hours.

FIG. 1.



three and ninety-nine hours respectively. The spleen of A contained rather few bacilli, either isolated or arranged in chains, which were never longer than are occasionally met with in control rabbits after simple inoculation with anthrax. In the lymph gland near the seat of inoculation, however, they were often found in much longer chains, or arranged in thick clusters. Of these chains, individual joints often were swollen up or refused to take the stain. The bacilli in B exhibited precisely similar appearances, but the chains of bacilli were somewhat longer, occasionally consisting of twenty-seven or twenty-eight joints.

In another series of experiments the rabbits were infected by intravenous injection of the blood of a rabbit dead of virulent anthrax and diluted with normal salt solution. Before or after this treatment, 5—10 c.c. of cell globulin solution were injected intravenously. After infection in this way the control rabbit died in twelve to eighteen hours. The rabbits treated with the globulin occasionally lived for a few hours longer, more often died at about the same time as the control. The chains of bacilli in the spleen sometimes exhibited a

curious appearance. They were surrounded by a transparent sheath, which was thickened at each junction between each two neighbouring bacilli. The chain of bacilli could be compared to a bamboo in which the nodes are swollen to nearly twice the diameter of the rest of the stick. A precisely similar appearance has been described and figured by Petruschky.* He found that anthrax bacilli undergo this change, which he regards as degenerative, when placed under the skin of frogs for several hours. Gamaleia† describes a somewhat similar condition as occasionally happening to anthrax vaccins in different organs of sheep and rabbits during the "vaccinal fever."

The above-described experiments would seem to lead to the following conclusions:—

1. That Halliburton's cell globulin-β possesses a bacteria-killing power.
2. That this power appears to distinguish it from fibrin ferment.
3. That this bacteria-killing power is of the same nature as that possessed by blood serum, as described by Buchner, Nissen, and Nuttall.
4. That this power of the serum is probably due to the same or some allied substance.
5. That, inasmuch as it is possible to obtain from cells that are, or can become, phagocytes a substance having bacteria-killing powers, we may suppose that phagocytes can not only kill microbes that they have ingested, but also do this, or tend to do this, by breaking down and liberating their contents.

The Society adjourned over the Whitsuntide Recess to Thursday, June 5th.

Presents, May 22, 1890.

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* "Die Einwirkungen des lebenden Froschkörpers auf den Milzbrandbacillus," 'Zeitschrift für Hygiene,' vol. 7, 1889, p. 75.

† *Loc. cit.*

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Two Bronze Medallion Portraits of Sir Christopher Wren and Daniel Wray, Fellows of the Royal Society. Mr. Evans, Treas. R.S.

June 5, 1890.

The Annual Meeting for the Election of Fellows was held this day.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Statutes relating to the election of Fellows having been read, General Clerk and Dr. Gladstone were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society:—

Baker, Sir Benjamin, M.Inst.C.E.
Bosanquet, Robert Holford Macdowall, M.A.
Burbury, Samuel Hawkesley, M.A.
Gardiner, Walter, M.A.
Kerr, John, LL.D.
Lea, Arthur Sheridan, D.Sc.
MacMahon, Percy Alexander, Major R.A.
Norman, Rev. Alfred Merle, M.A.
Perkin, Professor William Henry, jun., F.C.S.
Pickering, Professor Spencer Umfreville, M.A.
Roberts, Isaac, F.R.A.S.
Sharp, David, M.B.
Teall, J. J. Harris, M.A.
Thorne, Richard Thorne, M.B.
Weldon, Walter Frank Raphael, M.A.

Thanks were given to the Scrutators.

June 5, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Account of recent Pendulum Operations for determining the relative Force of Gravity at the Kew and the Greenwich Observatories." By General WALKER, C.B., F.R.S., LL.D. Received April 15, 1890.

(Abstract.)

It is well known that a series of pendulum observations was carried on in India, during the years 1865 to 1873, with two invariable pendulums, the property of the Royal Society. The Observatory of the Royal Society at Kew was chosen as the base-station of the operations, and the pendulums were swung there before being sent out to India, and again on their return from India. With a view to connecting the observations with those which had already been taken with other pendulums in other parts of the world, it was intended, on the return of the pendulums from India, to swing them at the Royal Observatory at Greenwich, which was a well established pendulum station, observed at by General Sir Edward Sabine, the Russian Admiral Lütke, and others. But when the time arrived for making the observations at the Greenwich Observatory, such extensive preparations were being made there for the equipment of expeditions for the observation of the approaching transit of Venus that no room was available for the pendulum operations. It was, therefore, decided to make the connexion with Kew by swinging at Kew Kater's convertible pendulum, for determining the absolute length of the seconds pendulum, which had been swung 40 years previously at Greenwich by General Sabine. This being done, the length of the seconds pendulum at Kew was found to be 0·0027 of an inch greater than the length which had been previously determined at Greenwich, and consequently that the daily vibration number was three vibrations greater at Kew than at Greenwich. The difference, however, was far too large to be admissible, as the observatories are nearly in the same latitude, and differ very slightly in height.

In 1881 Colonel Herschel, R.E., was deputed by the Secretary of State for India to take pendulum observations at the two observatories, and at the old pendulum station in London, and also at some stations in America, with a view to improving and strengthening the connexion between the observations in India and those in other parts of the world. On completing his work in America, he handed over the three pendulums which he had employed to officers of the United States Coast and Geodetic Survey, by whom they were taken round the world and swung at Auckland, Sydney, Singapore, Tokio, San Francisco, and finally at Colonel Herschel's terminal station at Washington.

But when the observations came to be finally reduced, it was found that the difference between Colonel Herschel's results at Kew and Greenwich, as shown independently by the three pendulums, had an extreme range of about seven vibrations in the daily vibration number. The cause of these differences was mysterious and inexplicable, and there was no alternative but to swing the pendulums a second time at the two observatories.

The revisionary work was undertaken by the observatory staff at each place, in such intervals of leisure as they could obtain from their regular operations. The final results, by the three pendulums, make the vibration number at Kew in excess of that at Greenwich by 1·56, 1·50, and 0·59, giving an average excess of 1·22.

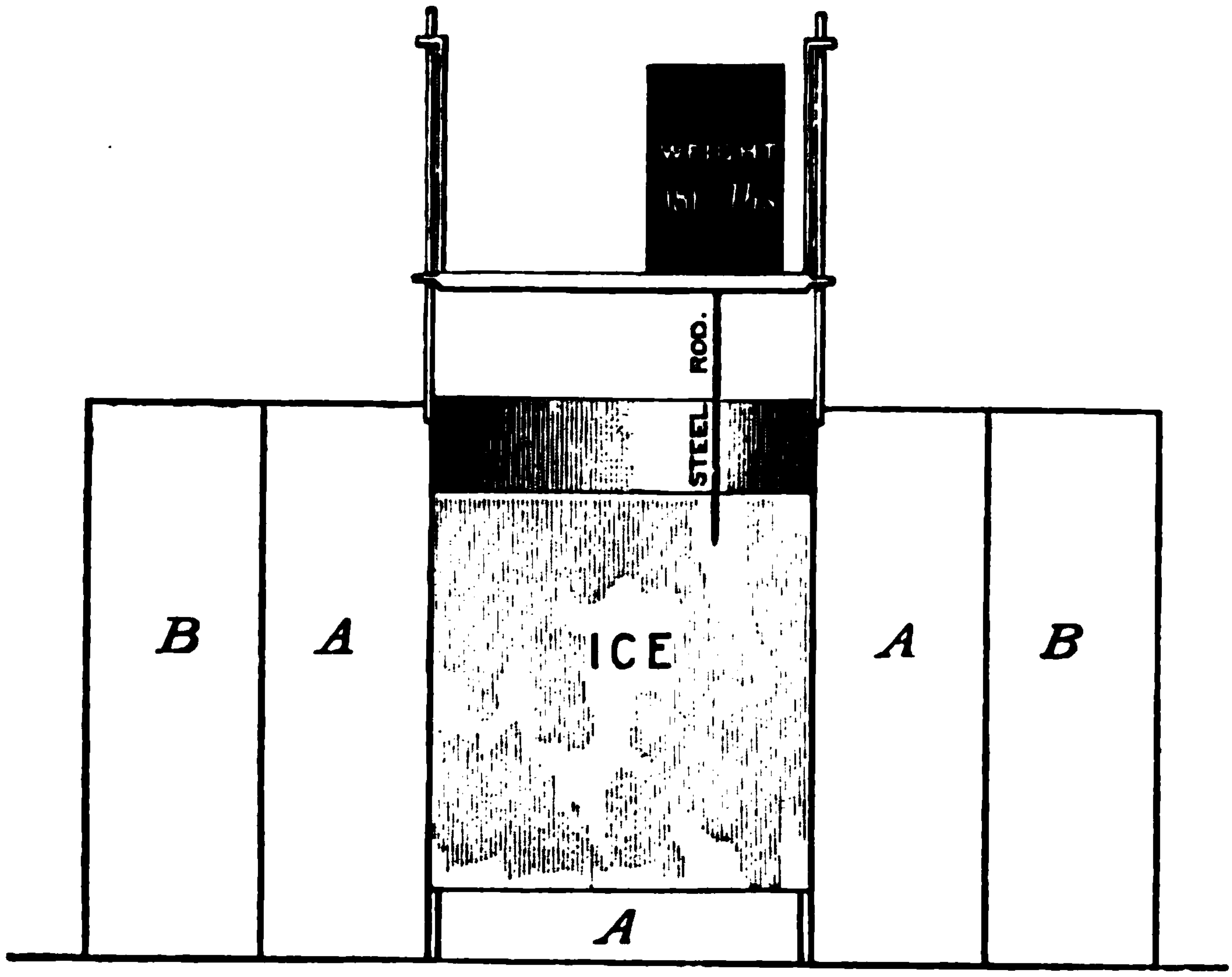
The correction to this quantity for the excess of height of the Greenwich over the Kew Observatory is —0·58. Thus, the revisionary operations, reduced to the mean sea-level, make the excess of Kew over Greenwich = 0·64 of a vibration, which may be accepted as very fairly probable.

II. "Observations on Pure Ice."—Part II. By THOS. ANDREWS, F.R.S., M.Inst.C.E. Received May 1, 1890.

The Plasticity of Ice.

In a paper, 'Roy. Soc. Proc.,' vol. 40, 1886, p. 544, I recorded the result of "Observations on Pure Ice and Snow," and having subsequently had occasion to use large quantities of low temperature freezing mixtures in the prosecution of other investigations, it seemed desirable to take advantage of the opportunity, and to further utilise the freezing mixtures in making collaterally the following additional experiments bearing on some of the plastic or viscous properties of ice at various temperatures. Messrs. J. C. McConnel and D. A. Kidd, in their valuable and interesting paper on "The Plasticity of Glacier and other Ice" ('Roy. Soc. Proc.,' vol. 44, 1888, p. 331), remark that "the variation of the plasticity of ice with the temperature is of great interest both for the theory of glaciers and for the explanation of the plasticity itself." I hope, therefore, that the experiments now recorded may assist in affording some information in connexion with this subject. An acquaintance with the causes of the flow of glaciers can scarcely be complete without some accurate experimental knowledge of the plasticity of ice at various temperatures, and it was partly with this object that the following experiments were commenced. The experiments form a continuation of those contained in my former paper. The arrangement of apparatus is described below, and illustrated by the accompanying sketch, fig. 1.

FIG. 1.



The ice for the pure ice experiments was frozen from distilled water contained in a sheet-iron vessel. The inner tank was surrounded both at the sides and bottom by an outer jacket of iron, which was provided with holes at the bottom for the waste liquids to escape. The coolest freezing mixture used, No. 1, consisting of three parts by weight of crystallised calcium chloride and two parts by weight of snow, was placed in the compartment A, which yielded a constant temperature of —35° F. There was also provided a larger outer compartment B, filled with the freezing mixture No. 2, of snow and ordinary salt, giving a steady temperature of 0° F., the latter mixture, No 2, completely enclosing the inner and coolest freezing preparation, No. 1. Good results were obtained by using this arrangement. Previous to mixing, large quantities of the snow and calcium chloride crystals were stored and maintained separately at a temperature of 0° F. in other vessels, and the respective freezing mixtures, No. 1 in compartment A, and No. 2 in compartment B, surrounding the ice tank were constantly renewed during the experiments from these cold stores. Thermometers were inserted in the mixtures and also in the ice during the experiments. The preceding description refers to the maintenance of the ice block at the lower temperature of —35° F. for the observations required at that temperature.

For the observations at 0° F. the ice block was surrounded only by the freezing mixture of snow and salt placed for these experiments in compartment A, and the ice block was encircled by snow only for

the experiments at 32° F. On repeating the experiments in every instance, the water for the ice block was first frozen by the application of a temperature of 0° F., which was afterwards reduced to —35° F. In making the observations, the extent of the penetration of the steel rod at the latter low temperature was first taken; the ice was then allowed gradually to acquire throughout a temperature of 0° F., and its penetrability ascertained; the ice block was afterwards allowed to reach the temperature of 28° and 32° F., and the final measurements obtained. The results recorded are the average of numerous measurements taken directly at different places on the ice block a sufficient distance apart, the indentation caused by the previous penetration of the rod being filled with water, which rapidly froze up. The variation in penetrability at different places on the ice block when measurements were taken at the same temperature was not great.

The polished steel rod used for ascertaining the penetrability was 16 inches long and 0·292 inch diameter; its extremity was a flat disk, so as to avoid any liability of shearing action. The rod was maintained in an upright position by a suitable arrangement of guides, and error arising from its conductivity was obviated by surrounding it throughout its length with fine sawdust contained in a loosely fitting bag which surrounded it. The weight, of 181½ lb. (inclusive of the weight of the platform and guides), was placed on a sliding wood platform working in a frame, the whole weight resting during experimentation on the top of the steel rod (see fig. 1).

Repeated observations were made in the above manner with the results recorded on Diagram I.

I also made a large number of experiments on the plasticity of natural lake or pond ice, which were conducted in a somewhat similar manner; the results showed a greater absence of uniformity in the amount of penetration when compared with results at a similar temperature obtained from the specially prepared pure ice blocks. The observations were taken on the surface of the ice of a large artificial lake or dam, being a storage reservoir for Wortley Iron Works, about 4 acres in extent, the depth varying from about 8 to 10 feet, the thickness of the ice being given on Diagram II. The water of this pond is still, no current of water passing through it; it is therefore practically a small lake. The observations were taken after sunset, and mostly throughout the long cold nights, so as to avoid, as far as possible, any influence of the sun on the surface of the pond ice. A number of measurements were taken adjacently in one locality of the pond, the apparatus being removed to another part for another set of observations, and so on, till the completion of the results. The observations were taken at various periods extending over some time. Thermometers were inserted in the ice, and the atmospheric tem-

perature was also regularly taken; the latter corresponded with the temperature of the ice. The results are given on Diagram II. In connexion with the influence of the composition of water on the plasticity of the ice frozen therefrom, it may be desirable to give an analysis of the water supplying the pond, which was as under:—

Table I.—Analyses of Water.

Results in Grains per Gallon.

	Total residue.	Inorganic matter.	Loss on ignition.	Total sulphates as (SO_3).
Dry seasons...	15·68	11·70	3·58	3·29
Rainy seasons.	11·31	7·56	3·75	2·93

Analysis of sample of water, during a very dry season.
Results in grains per gallon.

Deposit on boiling	0·30	
Iron (Fe)	0·28	
Calcium (Ca)	3·29	
Magnesium (Mg)	1·44	
Chlorine (Cl)	0·69	
(SO_4)	2·88	= SO_3 2·4
	8·88	
Total inorganic matters ...	8·40	

The above analyses of the water during dry seasons are the average of nine different analyses at periods extending over six years, and the analyses of the water during rainy seasons are the average of ten analyses at various periods during six years.

The reaction of the water with litmus was slightly acid, and sulphates were generally present in quantity.

A number of experiments were made to ascertain whether any portion of the saline constituents of the pond water was taken up into the ice during crystallisation. Contrary to expectation, and instead of the ice being found perfectly pure and free from saline matters, it was noticed that the composition of the natural ice was materially affected by the presence of a proportion of the salts of the water from which the ice was crystallised. Roughly speaking, the proportion of inorganic matter found in the melted ice would be about 10 per cent. of the total inorganic salts contained in the pond water from which it was frozen; it was also observed that there was a great propor-

tionate preponderance of organic matter in the ice compared with the water. The experiments were made as follows:—Portions of the ice were taken from numerous places on the pond, and very thoroughly washed with distilled water, the ice was then melted in large glass beakers, and the melted ice was found to be perfectly clear and translucent, there being no deposit after the water had stood for a considerable time. The melted ice, in quantities of half a gallon, was evaporated to dryness in a platinum basin, the residue dried at 212° and weighed, being subsequently ignited and again re-weighed. The results are given on Table II. The results obtained confirm the observations made by Buchanan on the composition of ice in Arctic seas; see remarks hereon towards the close of this Memoir.

Table II.—Analyses of Pond Ice.

Results in Grains per Gallon of the Melted Ice.

Experiment No.	Total residue.	Inorganic matter.	Loss on ignition.	Examination of the inorganic matters. Result in grains per gallon of melted ice.	
1	1·70	0·92	0·78		
2	1·84	1·10	0·74	Iron (Fe)......	0·110
3	1·86	0·90	0·96	Calcium (Ca)...	0·207
4	1·80	0·94	0·86	Magnesium (Mg)	0·0615
5	1·42	0·84	0·58	SO_4...........	0·461 = SO_3 0·385
6	1·47	0·91	0·56		
Average..	1·68	0·93	0·75	Chlorides were also present.	

The penetration of the steel rod into pure ice at constant temperature of −35° F. is shown on Diagram I by curve I; at 0° F. by curve II; at 28° F. by curve III; and at 32° F. by curve IV.

In the experiment, curve II, the needle was allowed to remain on the ice for a total period of 42 hours, the penetration at 18 hours from commencement was 0·906 inch, at 30 hours 1·031 inch, and at 42 hours from commencement 1·187 inch.

The penetration of the steel rod into pond ice at 28° F. is shown on Diagram II by curve I, and at 32° F. by curve II.

The pond ice, curve I, was 6¼ inches thick, and curve II, 5⅛ inches thick.

DIAGRAM I.—Plasticity of Pure Ice, as shown by penetration of steel rod therein, at temperatures stated.

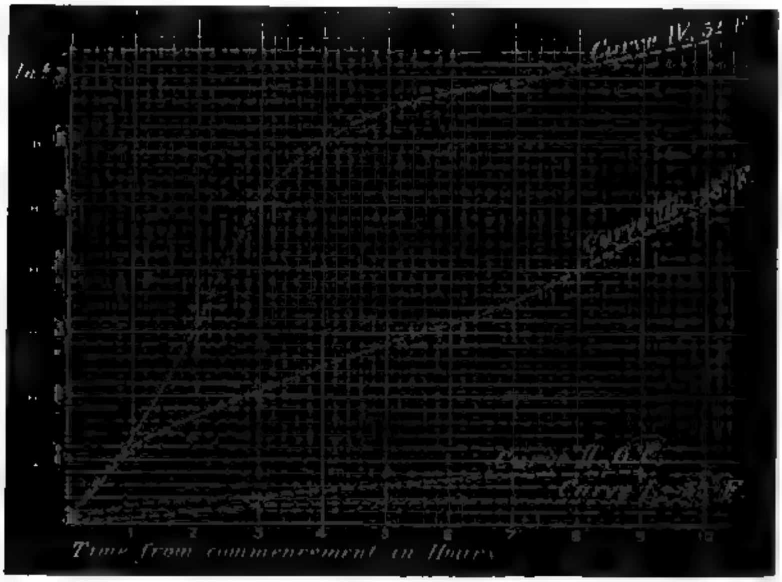


DIAGRAM II.—Plasticity of Pond Ice, as shown by penetration of steel rod therein, at temperatures stated.

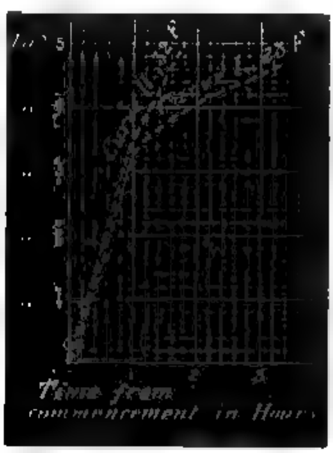


Table III.

Penetrability of steel rod into pure ice. Temperature of ice rising gradually from 0° to 32° F.		
Time from commencement.	Temperature of ice in degrees Fahrenheit.	Penetration of steel rod.
		inches.
3 hours.	10·00	0·458
4 "	12·33	0·604
5 "	14·00	0·781
6 "	17·33	0·948
7 "	19·00	1·125
8 "	20·00	1·271
9 "	20·67	1·427
10 "	21·33	1·604
11 "	22·00	1·750
12 "	22·67	1·937
13 "	23·33	2·104
14 "	24·33	2·250
15 "	24·67	2·448
16 "	25·33	2·635
17 "	25·67	2·777
18 "	26·33	2·888
20 "	26·67	3·124
21 "	27·33	3·339
22 "	27·67	3·502
23 "	28·00	4·021
26 "	28·33	4·423
27 "	29·00	4·806
29 "	29·67	5·174
31 "	30·00	5·583
34 "	31·33	6·375
35 "	31·67	6·661
38 "	31·67	7·155
39 "	31·67	7·483
51 "	32·00	8·625
89 "	32·00	10·125

The above results are the average of three sets of observations made on a cylinder of pure ice 2 feet $1\frac{1}{2}$ inches long, 2 feet $1\frac{1}{2}$ inches diameter, weighing 470 lbs.

Table IV.

Time from commencement.	Penetration of steel rod into pure ice, the ice gradually rising in temperature from 26° to 32° F., and then remaining at 32° F., and subsequently gradually softening.					
	Experiment No. 1.		Experiment No. 2.		Experiment No. 3.	
	Tempera- of ice in degrees Fahr.	Penetration of steel rod No. 1 in inches.	Tempera- of ice in degrees Fahr.	Penetration of steel rod No. 2 in inches.	Tempera- of ice in degrees Fahr.	Penetration of steel rod No. 3 in inches.
hrs. m.						
0	26	0·000				
5	26	2·750				
15	26	3·000				
30	26	3·250				
45	27	3·500				
1 0	28	3·750				
2 0	28	4·125				
3 0	28	4·375				
4 0	28	4·625				
8 0	28	5·375				
13 30	28	6·562				
19 0	28	7·750				
46 20			32	0·000		
46 25			32	5·000		
46 40			32	5·250		
46 55			32	5·562		
47 10			32	5·875		
47 25			32	6·062		
47 40			32	6·187		
48 40			32	6·562		
48 45					32	0·000
48 50					32	5·375
48 55					32	6·562
49 5					32	7·000
49 10					32	8·500
49 15					32	9·500

The above observations were made on a cylinder of pure ice 2 feet 1½ inches long, 2 feet 1½ inches diameter, and weighing 470 lbs.

In experiment No. 2, the ice cylinder had remained at a temperature of from 26° to 28° F. for 46 hours 20 minutes previous to commencing the experiment with steel rod No. 2.

In experiment No. 3, the ice cylinder had remained at a tempera- tuure of 32° F. for 2½ hours previous to commencing the experiment with steel rod No. 3.

These experiments show that the plasticity of the ice was very

rapidly and considerably increased after reaching, and whilst it afterwards remained at, a temperature of 32° F.

General Remarks.

1st. Referring to the results of Diagram I, and taking the relative penetration of the steel rod at the respective temperatures as an indication of the plasticity of the ice, it will be noticed that there was a considerable reduction of plasticity as the temperature lowered. Regarding as a guide the total depth penetrated by the steel rod, during equal and comparative periods of time (as, for instance, the results at 2, 3, 4, 5, 6, 7, &c., hours, Diagram I, curves 1, 2, 3), into ice at different temperatures, these comparative experiments show, in a majority of instances, that, if the plasticity of the ice at −35° F. be called one, at 0° F. it would be about twice as much, and at 28° F. the plasticity would be about four times as great as at 0° F., or eight times as much as at −35° F. When the ice was maintained at a temperature of 32° F., it will be seen that the plasticity very considerably increased, this being probably owing to the reduced cohesion at this temperature between the faces of the ice crystals forming the mass.

2nd. It will be further noticed that the plasticity was greater during the gradual molecular changes occurring in course of the slow rising of temperature from 0° F. to 32° F., see results on Table III, than when the ice was maintained at even temperatures, as in the experiments on Diagram I.

3rd. Reverting to the observations on Table IV, if the time required for the steel rod to penetrate a certain depth, say, 6½ inches, into the ice under conditions of experiments 1, 2, and 3, Table IV, be taken as an indication of the relative plasticity of the ice under conditions of these experiments, it will be seen that the plasticity was roughly about 579 per cent. greater in No. 2 than No. 1, and 1400 per cent. greater in No. 3 than in No. 2, these results showing proportionately the rapid manner in which the mass of ice was becoming internally plastic, although retaining an outward apparent rigidity. The Rev. Coutts Trotter, in his paper ('Roy. Soc. Proc.,' vol. 38, 1885, p. 92), states that "the 'viscosity' of ice probably diminishes very rapidly with the temperature," and it appears probable from the experiments of M. Person ('Comptes Rendus,' vol. 30, 1850, pp. 526—528) that −2° C. is the temperature at which ice begins to soften. The Rev. C. Trotter, after summarising the experimental evidence, also arrives at the conclusion that the general interior and bottom portions of a glacier, near the surface of the earth, are of a constant temperature of about 0° C., and my experiments recorded on Diagrams I and II and Tables III and IV demonstrate the greater

plasticity of ice, in the mass, at similar temperatures. Mr. Trotter further remarks that "the supposition that, while ice at 0° C. is sensibly viscous, the viscosity diminishes rapidly with the temperature is in complete accordance with the facts of the changes which take place in a glacier during the winter." The comparatively great contractibility in ice observed at considerably reduced temperatures, see my paper on "Observations on Pure Ice and Snow" ('Roy. Soc. Proc.,' vol. 40, 1886, p. 544), accounts for the great reduction in its plastic properties. This is in full accord with the practical cessation of motion in glaciers during the cold of winter. I believe that the plastic properties of ice in the mass are variable, and are also, to some extent, influenced by the rapidity or otherwise of its crystallisation. Thus, a mass of water rapidly frozen at an intensely low temperature would, no doubt, crystallise into a larger number of smaller crystals than those which would result from a more slow solidification of the mass of water at a comparatively higher temperature of freezing.

4th. It will be noticed, on comparing the results of Diagrams I and II, that the plasticity of the naturally frozen pond ice was manifestly greater than that of the artificially prepared pure ice, and the difference in results may, to a certain extent, probably be accounted for by difference of composition of the respective frozen waters and ice (the block ice being frozen from pure distilled water, the composition of the pond water and ice frozen therefrom being given on Tables I and II); I think, also, the difference was, to some extent, owing to the direction from which the pond ice was frozen, viz., from the surface only. Further, this comparative difference in the behaviour of the pond ice was doubtless owing to a portion of the saline constituents of the water interspersing, during congelation, between the faces of the individual crystals of ice, thereby tending to reduce the cohesion of the mass as a whole and increasing its plasticity. Mr. J. Y. Buchanan, F.R.S., has shown that in Arctic ice, contrary to expectation, the whole of the salts do not separate from sea-water ice during congelation, but that they remain interspersed amongst the interstices of the crystalline mass ("Ice and Brine," 'Edinburgh Roy. Soc. Proc.,' vol. 14, 1888, p. 129). My experiments on Table II also show that the composition of pond or natural river ice is affected in a similar manner, and afford confirmation of the views held by Buchanan on the composition of sea-water ice in Arctic regions. The fact of the pond ice having been slowly crystallised would further tend to modify its physical properties, compared with ice rapidly crystallised by an intensely low temperature, simultaneously acting only from the bottom and sides of the mould or tank. Measurements of the expansibility of ice may also be affected accordingly as such measurements are taken either longitudinally or

transversely to the line of the cold crystallising force, and from my former experiments in this direction ('Roy. Soc. Proc.,' vol. 40, 1886, p. 544), I think there appear substantial indications that ice may expand unequally in different directions. Messrs. McConnel and Kidd have shown, in their experiments with glacier ice, that "ordinary ice, consisting of an irregular aggregation of crystals, exhibits plasticity, both under pressure and under tension at temperatures far below the freezing point, down to $-9°$ at least, and probably much lower." The experiments recorded on Diagram I now practically demonstrate the latter supposition, and I found the plasticity at the lower temperature to be very considerably reduced. Mr. J. Y. Buchanan, F.R.S., in his paper on "Ice and Brine" ('Edinburgh Roy. Soc. Proc.,' vol. 14, 1888, p. 129), has expressed notions of the plasticity and flow of glacier ice which tend to confirm the views of Messrs. McConnel and Kidd on this subject. In this direction the experiments on pure ice, Diagram I, compared with those on pond ice, Diagram II, have shown that ice frozen from water containing saline constituents is more plastic than the ice frozen from pure distilled water.

I hope that the experiments of this memoir may help to afford information in connexion with the interesting subject of the plasticity of ice.

Appendix.

Attention has very recently been drawn to the manner in which lake ice has a tendency to crystallise, in a series of interesting letters published in 'Nature,' 1889, by Mr. Thomas H. Holland, Mr. T. W. Backhouse, Mr. T. D. Latouche, Messrs McConnel and Kidd, and others. I have myself also frequently noticed the six-rayed starlike figures and skeleton triangular forms on natural pond ice, and other similar indications of the tendency of lake ice to the hexagonal form of crystallisation.

III. "The Passive State of Iron and Steel."—Part I. By Thos. Andrews, F.R.SS.L. and E., M.Inst.C.E. Received May 18, 1890.

The singular metallurgical phenomenon of the passive state of iron presents many features of interest, affording a wide field for original research. The knowledge we possess on this peculiar and obscure subject is not, however, very extensive, owing possibly to the difficulties encountered in devising suitable methods of research in relation thereto. The author, therefore, approached the investigation with considerable diffidence, feeling greatly the difficulties accompanying

a research of this intricate nature. He feels, however, rewarded by the measure of success which has ensued. The general tentative conclusion he has arrived at, as a result of careful experimentation, is that the passive state of iron and steel ought not to be regarded as fixed or static, the electro-chemical observations tending to show that the passivity is a property influenced more or less by various conditions, such as variation of the molecular structure and chemical composition of the iron and steel, different strengths of nitric acid, modification of attendant physical conditions, magnetism, temperature, &c. It is known that when bright iron is immersed in nitric acid of 1·4 sp. gr., the iron is not acted upon, but remains passive in the acid, which appears to exert no perceptible effect upon the metal. "Under certain circumstances iron is not acted upon at all by nitric acid. Iron in this state is termed passive, and this condition is brought about by dipping the metal into concentrated nitric acid and then washing it" ('Treatise on Chemistry,' by Sir Henry E. Roscoe, F.R.S., and Dr. C. Schorlemmer, F.R.S.). The late Dr. Jno. Percy, F.R.S., the eminent metallurgist, referring to the passive state of iron, remarked that "this is a very curious and interesting subject, which may possibly be one day found to admit of valuable practical application." The passive state of iron appears first to have been observed just a century ago by Keir, and brought before the notice of the Royal Society in 1790 ('Phil. Trans.,' 1790, p. 379); he observed that strong nitric acid had no action on iron when the metal was placed therein. Bergman, Scheurer-Kestner, Schönbein, and Buff also made some observations on the passivity of iron previous to the year 1848. Faraday and Beetz were disposed to attribute the passive state of iron to the immediate formation of a very fine envelope or film of oxide on immersion of the metal in concentrated nitric acid, whereas Sir John Herschel ('Annales de Chimie,' vol. 54, 1833, p. 87) considered the phenomenon was due to a certain permanent electric state of the surface of the metal.

Westlar ('Annales des Mines,' vol. 2, 1832, p. 322) observed that when iron or steel had been immersed in nitrate of silver solution it failed to precipitate copper from its solutions.

Braconnot ('Annales de Chimie,' vol. 52, 1833, p. 288) noticed that filings or plates of iron were not at all affected in strong nitric acid at ordinary temperatures.

In the present memoir are presented the results of a study of certain magnetic, temperature, and other conditions which the author found to affect the passive state of iron and steel, and he is extending the inquiry into the relative passivity of the various modern steels and other aspects of the subject. The author observed, in course of experimentation some years ago, that when bright iron or steel was magnetised whilst immersed in concentrated nitric acid, its passive

condition was to some slight extent modified, magnetism acting through considerable periods of time apparently exerting a measurable influence. For conducting the present investigation he devised, and, after careful consideration, decided to adopt, the delicate electrochemical method of research hereafter described, and by the use of a sensitive astatic mirror galvanometer and other appliances the following results were obtained, which it is hoped may prove of interest and afford an addition to our knowledge of some of the conditions affecting the passive state of iron and steel.

For convenience, the experiments are classified under the following heads:—

Series I, Table I, containing the result of observations on the influence of magnetisation on the passive state of steel in cold nitric acid, sp. gr. 1·42, at temperature of 45° F. or less.

Series II, Table II, treating of the influence of magnetisation on the passive state of steel in warm nitric acid, sp. gr. 1·42, above the temperature of 45° F., the experiments showing that magnetised steel bars were less passive in warm nitric acid than unmagnetised ones.

Chemical Analysis of the Steel Bars.

	per cent.
Combined carbon	0·570
Silicon	0·032
Sulphur	trace
Phosphorus	0·066
Manganese	0·147
Iron (by difference)	99·185
Total	100·000

Physical Properties of the Steel Bars.

Contraction of area at fracture.	Extension.	Breaking strain per square inch of original area.
per cent. 22	per cent. 2·0	tons 55·42

Series I.

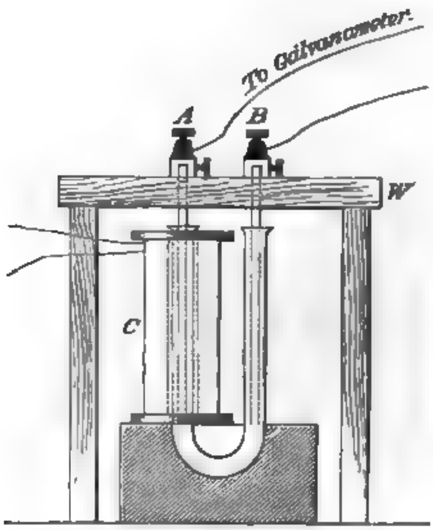
The Influence of Magnetisation on the Passive State of Steel in Cold Nitric Acid.

The method of experimentation for the observations of Series I was as follows:—

For each experiment a pair of polished steel bars, $8\frac{1}{2}$ inches long, and 0·261 diameter, were cut adjacently from a long steel rod to insure practical uniformity of structure and composition. The steel and iron rods were of the chemical composition and physical properties given on previous page.

The apparatus will be understood on reference to fig. 1.

FIG. 1.



A pair of bars, supported in a wooden frame, W, were immersed in $1\frac{1}{4}$ fluid oz. of cold concentrated nitric acid, sp. gr. 1·42, contained in the U-tube, A being the magnetised bar, and B the unmagnetised one, and allowed to remain therein for periods stated on the Table I. The bars were in circuit with a delicate astatic mirror galvanometer, the telescopic observations of the deflections of which were carefully taken. In some cases the bar A was previously permanently magnetised by hand, and in other instances the bar was magnetised in the coil C for a short time. In the latter instances the subsequent results were due to the residual permanent magnetism. The magnetising coil, C, was a powerful one, and was worked in connexion with a bichromate battery in cases where the magnetisation of the metal was made in the coil.

A considerable number of experiments were made in the above manner, those sets recorded on the Table I being selected as average typical ones. In all the following experiments a new wooden stand and a fresh pair of bright polished steel or iron rods were prepared for each set of observations.

The E.M.F. recorded in all the following tables was ascertained by using a sensitive galvanometer of known calibration in conjunction with the ascertained resistance of the nitric acid solutions.

Table I.

Influence of Magnetisation on the Passive State of Steel.

Time from commencement of experiment.	Electro-chemical effect between magnetised and unmagnetised steel bars immersed in circuit in cold nitric acid, sp. gr. 1·42. E.M.F. in volt. The electro-chemical position of magnetised bar, positive, except where otherwise specified.					
	Experiments, Set I.	Experiments, Set II.	Experiments, Set III.	Experiments, Set IV.	Experiments, Set V.	Experiments, Set VI.
seconds.						
0		0·022	0·022		0·016	
15		0·013	0·021		0·014	
30		0·011	0·020		0·013	
minutes						
1	0·001	0·008	0·019	0·008	0·013	
4	0·004	0·007	0·009	0·011	0·014	
6	0·004	0·008	0·006	0·011	0·014	
8	0·004	0·008	0·007	0·011	0·015	
10	0·005	0·007	0·008	0·011	0·015	0·006
12	0·005	0·007	0·009	0·011	0·015	0·006
16	0·005	0·007	0·011	0·011	0·015	0·006
20	0·006	0·006	0·011	0·011	0·015	0·006
24	0·006	0·006	0·012	0·010	0·515	0·006
28	0·006	0·006	0·013	0·010	0·015	0·006
30	0·006	0·006	0·013	0·010	0·015	0·006
35		0·007	0·013	0·006	0·015	0·006
50		0·008	0·014	0·007	0·015	0·004
hours						
1		0·010	0·014	0·007	0·016	0·004
2		0·011	0·018	0·007	0·016	0·004
3		0·013	0·020	0·008	0·017	0·005
3½		0·016	0·022	0·008	0·017	0·005
4			0·022	0·008	0·017	0·006
6			0·024		0·017	0·006
8			0·026		0·018	0·007
12			0·030		0·018	0·007
16			0·030		0·018	0·007
18			0·030		0·018	0·008
20			0·029		0·019	0·008
22			0·029		0·019	0·008
24			0·029		0·019	0·008
26						0·007
28						0·007
30						0·007
40						0·013
42						0·013

Table I—*continued.*

Influence of Magnetisation on the Passive State of Steel.

Time from commencement of experiment.	Electro-chemical effect between magnetised and unmagnetised steel bars immersed in circuit in cold nitric acid, sp. gr. 1·42. E.M.F. in volt. The electro-chemical position of magnetised bar, positive, except where otherwise specified.					
	Experiments, Set VII.	Experiments, Set VIII.	Experiments, Set IX.	Experiments, Set X.	Experiments, Set XI.	Experiments, Set XII.
hours						
1		0·0005 N	0·0005 N	0·000	0·000	0·011
3	0·004 N	0·006	0·001 N	0·000	0·000	0·018
12	0·003	0·011	0·000	0·002	0·002	0·020
20	0·008	0·012	0·001	0·004	0·004	0·021
24	0·012	0·012	0·001	0·002	0·005	0·026
26	0·012	0·012	0·001	0·002	0·005	0·026
28	0·013	0·013	0·002	0·002	0·006	0·026
30	0·013	0·013	0·002	0·002	0·006	0·028
32	0·013	0·014	0·002		0·006	0·028
40	0·016	0·015	0·004		0·007	0·030
42	0·017	0·015	0·004		0·007	0·030
48	0·020	0·016	0·006		0·007	0·032
51	0·020	0·016	0·006		0·008	0·032
64	0·020	0·016	0·007		0·011	0·032
66	0·020	0·017	0·008		0·011	0·032
72	0·023	0·017	0·011		0·011	0·032
days						
3¼	0·023	0·017	0·011		0·011	0·030
3½	0·024	0·017	0·012		0·012	0·027
3¾	0·025	0·018	0·013		0·012	0·024
4	0·026	0·018	0·014		0·013	0·022
4¼	0·026	0·018	0·014		0·013	0·020
4½	0·027	0·018	0·015		0·013	0·018
4¾	0·027	0·019	0·015		0·013	0·016
5	0·028	0·019	0·016		0·013	0·013
5¼	0·028	0·019	0·016		0·013	
5½	0·028	0·018	0·017		0·013	
5¾	0·028	0·018	0·017		0·013	
6	0·028	0·017	0·017		0·013	
6¼	0·027		0·017		0·013	
6½	0·026		0·017		0·013	
6¾	0·025		0·017		0·013	
7	0·024		0·017		0·013	

Some of the magnetised bars employed in the experiments on Table I were magnetised by touch, and in other experiments they were magnetised in the coil as previously described.

In some instances on Table I it was noticed that on the first immersion of the metals in the nitric acid an almost instantaneous but steady deflection of considerable extent of the needle of the galvanometer occurred, indicating an electro-positive position for the

magnetised bar; this more extended fling of the galvanometer, however, subsided in a few seconds, and did not interfere with after results due to electro-chemical action.

The results of Series I, in cold nitric acid, were perhaps more perceptible in those experiments extending over the longer periods (see Table I), though even in these, the effect was comparatively small. In a recent research by the author on "Electro-chemical Effects on Magnetising Iron," Part II ('Roy. Soc. Proc.,' vol. 44, p. 152), it was noticed that local currents were set up between the polar terminals and central portions of steel magnets exposed in electrolytes, and this class of local action, together with the slight alteration of the physical structure of the magnet bars consequent on their magnetisation, may possibly be involved in producing the effects due to magnetism on passive steel or iron in concentrated nitric acid. The influence of magnetism of low intensity, whilst modifying to a very limited extent, does not, however, appear sufficient entirely to overcome, the passivity of the metal in nitric acid, and the influence of even a powerful magnetic force, though doubtless slightly modifying (to the extent, possibly, of the slight alteration of physical state in the metals, induced by magnetism), did not destroy the passivity of bright iron or steel exposed to its action in *cold* strong nitric acid.

The whole of the results on Table I afford an indication that magnetisation of comparatively low intensity, acting during considerable periods of time, exerts only a limited modifying influence on the passivity of iron or steel in the cold, or up to a temperature of about 60° F., though the influence is just discernible when employing a delicate galvanometer. Magnetisation, with the nitric acid at a higher temperature, produces a quicker effect, see results in Series II, Table II. The author is pleased to see that somewhat similar conclusions as to the influence of powerful magnetisation on the passive state of powdered iron in *warm* nitric acid have been independently arrived at in America by Messrs. Nichols and Franklin, who, in some recent interesting experiments, have found that powdered iron in nitric acid, 1·368 sp. gr., when placed in a test-tube in a suitable apparatus between the poles of a powerful electro-magnet commenced to be violently acted upon when raised to a temperature of 51° C. Unmagnetised iron usually remains passive in strong nitric acid until a temperature of about 89° C. is reached, when an explosion, consequent on loss of passivity, occurs. Messrs. Nichols and Franklin's experiments, therefore, show that very powerful magnetic action helps to lower the temperature of transition from the passive to the active state. They have also found that "the intensity of the magnetic field necessary to convert passive into active iron at a given temperature increases rapidly with the concentration of the acid."

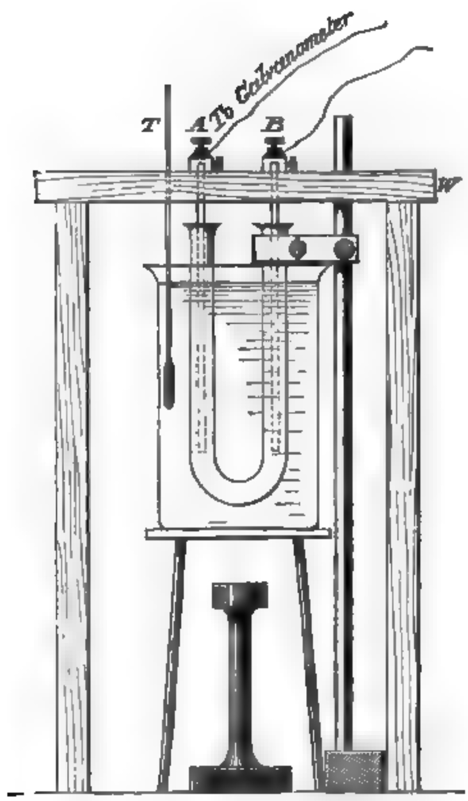
Unknown to each other, we have been simultaneously and independently prosecuting researches having a like object, and it is satisfactory to have arrived at somewhat similar conclusions, though by different methods of experimentation.

Series II.

Influence of Magnetisation on the Passive State of Steel in Nitric Acid, 1·42 sp. gr., above the temperature of 45° F.

The experiments for this set of observations were conducted in a somewhat similar manner to those of Series I. The U-tube contain-

Fig. 2.



ing the nitric acid of 1·42 sp. gr., with the magnetised bar A, $8\frac{1}{4}$ inches long, 0·261 inch diameter (permanently magnetised by touch), and unmagnetised bar B, $8\frac{1}{4}$ inches long, 0·261 inch diameter,

were immersed and rigidly supported in a large beaker of water on a sand-bath, and the whole gradually heated to any required temperature, a thermometer, T, being suspended in the beaker. The magnetised and unmagnetised bars were in circuit with a delicate galvanometer throughout each experiment.

The arrangement is shown on fig. 2, and the results are given on Table II.

The nitric acid remained perfectly colourless until a temperature of about 100° to 120° F. was reached, when a very pale-yellow tint began to be perceptible in both tubes, which gradually deepened until the critical point of explosion was reached, at which the passivity of the steel ceased.

During the intervening period, the metal was acted upon but slightly in both tubes; a small evolution of gas showed itself in the form of tiny bubbles adhering to the surface of the steel rod at a temperature of about 170° F. No extensive solvent action, however, occurred until the acid arrived at a temperature of about 190° to 200° F. (the explosion occurred most frequently at an average temperature of 195° F.), when, without any previous warning, a violent explosive evolution of red nitric oxide gas took place, the steel being instantly and vigorously attacked by the nitric acid. After the first explosion the metal again became partially passive for a short time. The explosion and solution of the steel first commenced in the limb of the U-tube containing the magnetised steel bar, and the first intimation of the passivity of the steel having been overcome was a sudden and extensive fling of the galvanometer, positive for the magnet bar. The bars were almost instantly removed from the nitric acid; the solution in the limb of the U-tube which had contained the magnetised bar was of a very dark-brown colour compared with solution in the other limb, thus giving further proof that the magnetised bar had been first attacked.

At the moment of explosion, the E.M.F. was very considerable; in several instances it varied from about $\frac{1}{10}$th to as high as $\frac{1}{3}$rd of a volt, the magnetised bar being the electro-positive metal, see Table II.

In the experiment, Table II, col. 9, the temperature was not raised above 185° F., and no explosive evolution of nitric oxide gas, therefore, occurred; the metals were maintained in the nitric acid at a temperature gradually reducing from 185° to 165° F. in course of one hour. The steel did not fully lose its passivity, but was, however, only very slowly and partially acted upon by the nitric acid, even at this high temperature.

Table II.

Temperature in degrees, Fahrenheit, from commencement.	Current between bright magnetised and unmagnetised passive steel bars immersed in circuit in warm nitric acid, 1·42 sp. gr. The electro-chemical position of the magnetised bar, positive, except where otherwise specified. E.M.F. in volt.					
	Column No. 1.	Column No. 2.	Column No. 3.	Column No. 4.	Column No. 5.	Column No. 6.
55	0·005				0·001	
60	0·005	0·005	0·003	0·000	0·004	0·004
65	0·005	0·005	0·003	0·0005 N	0·005	0·004
70	0·006	0·005	0·003	0·001 N	0·006	0·004
75	0·006	0·005	0·003	0·001 N	0·008	0·003
80	0·006	0·005	0·002	0·001 N	0·009	0·003
85	0·007	0·005	0·002	0·001 N	0·011	0·002
90	0·009	0·005	0·002	0·001 N	0·011	0·001
95	0·009	0·006	0·002	0·001 N	0·012	0·0005
100	0·009	0·006	0·002	0·0005 N	0·013	0·000
105	0·009	0·006	0·002	0·000	0·012	0·0005 N
110	0·010	0·006	0·002	0·000	0·011	0·001 N
115	0·010	0·006	0·002	0·004	0·011	0·001 N
120	0·010	0·006	0·003	0·009	0·010	0·002 N
125	0·010	0·006	0·010	0·012	0·009	0·001 N
130	0·010	0·006	0·014	0·014	0·009	0·001 N
135	0·011	0·005	0·018	0·014	0·007	0·0005 N
140	0·014	0·004	0·023	0·014	0·006	0·000
145	0·013	0·003	0·023	0·014	0·006	
150	0·010	0·002	0·023	0·014	0·006	0·003
155	0·009	0·001	0·022	0·015	0·006	0·004
160	0·005	0·000	0·020	0·017	0·006	0·009
165	0·007	0·003	0·019	0·018	0·005	0·011
170	0·009	0·006	0·018	0·019	0·004	0·013
173	0·009	0·011	0·017	0·021	0·005	0·013
175	0·009	0·010	0·016	0·024	0·005	0·014
180	0·009	0·010	0·016	0·026	0·006	0·013
185	0·006	0·011	0·014	0·027	0·004	0·010
190	0·006	0·012	0·014	0·027	0·001	0·006
191	0·006*				0·000	
192	0·328	0·189*				
193			0·110*			
195					0·038*	
198						0·072*
200				0·038		
202				0·038		
205				*		
Temperature point of transition from passive to active state.						
	191° F.	192° F.	193° F.	205° F.	195° F.	198° F.

Table II—*continued.*

Temperature in degrees, Fahrenheit, from commencement.	Current between bright magnetised and unmagnetised passive steel bars immersed in circuit in warm nitric acid, 1·42 sp. gr. The electro-chemical position of the magnetised bar, positive, except where otherwise specified. E.M.F. in volt.				
	Column No. 7.	Column No. 8.	Column No. 9.	Column No. 10.	Column No. 11.
45	0·000				
50	0·0005	0·007	0·002	0·002 N	
55	0·001	0·007	0·002	0·003 N	
60	0·001	0·008	0·002	0·003 N	0·004
65	0·001	0·009	0·003	0·002 N	0·003
70	0·001	0·008	0·003	0·002 N	0·003
75	0·002	0·008	0·004	0·000	0·003
80	0·002	0·007	0·005	0·002	0·003
85	0·002	0·006	0·006	0·004	0·002
90	0·002	0·006	0·006	0·006	0·002
95	0·003	0·005	0·005	0·009	0·002
100	0·005	0·004	0·004	0·010	0·002
105	0·005	0·003	0·002	0·011	0·001
110	0·007	0·002	0·001	0·013	0·001
115	0·008	0·003	0·000	0·014	0·001
120	0·009	0·003	0·000	0·014	0·002
125	0·010	0·004	0·000	0·014	0·003
130	0·011	0·004	0·001 N	0·014	0·004
135	0·012	0·006	0·002 N	0·014	0·006
140	0·012	0·009	0·003 N	0·014	0·007
145	0·012	0·011	0·003 N	0·015	0·009
150	0·013	0·011	0·003 N	0·015	0·011
155		0·011	0·002 N	0·016	0·014
160	The temperature of the solution was, in these experiments, maintained at about 150° F. for one hour without explosion occurring, the E.M.F. gradually rising to 0·22 of a volt.		0·000	0·020	0·018
165			0·002	0·027	0·017
170			0·003	0·030	0·011
175			0·004	0·034	
180			0·007	0·038	
185			0·009	0·041	
190				0·072*	
Temperature point of transition from passive to active state.					
				190° F.	

In addition to the observations recorded in Table II, numerous other confirmatory experiments were made which it is not needful to give in detail.

* At these temperatures the first violent explosive ebullition of red nitric oxide gas occurred consequent on the loss of passivity of the metal. The average critical temperature point of transition from the passive to the active state was about 195° F.

The observations show that the magnetised steel bars were less passive in warm nitric acid than the unmagnetised steel bars.

IV. "On the Superficial Viscosity of Water." By LORD RAYLEIGH, Sec. R.S. Received May 15, 1890.

The idea that liquids are endowed with a viscosity peculiar to the surface is to be found in the writings of Descartes and Rumford; but it is to Plateau that its general acceptance is due. His observations related to the behaviour of a compass needle, turning freely upon a point, and mounted in the centre of a cylindrical glass vessel of diameter not much more than sufficient to allow freedom of movement. By means of an external magnet the needle was deflected 90° from the magnetic meridian. When all had come to rest the magnet was suddenly removed, and the time occupied by the needle in recovering its position of equilibrium, or rather in traversing an arc of 85°, was noted. The circumstances were varied in two ways: first, by a change of liquid, *e.g.*, from water to alcohol; and, secondly, by an alteration in the level of the liquid relatively to the needle. With each liquid observations were made, both when the needle rested on the surface, so as to be wetted only on the under side, and also when wholly immersed to a moderate depth. A comparison of the times required in the two cases revealed a remarkable dependence upon the nature of the liquid. With water, and most aqueous solutions, the time required upon the surface was about *double* of that in the interior; whereas, with liquids of Plateau's second category, alcohol, ether, oil of turpentine, &c., the time on the surface was about *half* of the time in the interior. Of liquids in the third category (from which bubbles may be blown), a solution of soap behaved in much the same manner as the distilled water of the first category. On the other hand, solutions of albumen, and notably of saponine, exercised at their surfaces an altogether abnormal resistance.

These experiments of Plateau undoubtedly establish a special property of the surfaces of liquids of the first and third categories; but the question remains open whether the peculiar action upon the needle is to be attributed to a viscosity in any way analogous to the ordinary internal viscosity which governs the flow through capillary tubes.

In two remarkable papers,* Marangoni attempts the solution of this problem, and arrives at the conclusion that Plateau's superficial viscosity may be explained as due to the operation of causes already recognised. In the case of water and other liquids of the first category, he regards the resistance experienced by the needle as

* 'Nuovo Cimento,' Ser. 2, vol. 5–6, Apr., 1872; 'Nuovo Cimento,' Ser. 3, vol. 3, 1878.

mainly the result of the deformation of the meniscuses developed at the contacts on the two sides with the liquid surface. This view does not appear to me to be sound; for a deformation of a meniscus due to inertia would not involve any dissipation of energy, nor permanent resistance to the movement. But the second suggestion of Marangoni is of great importance.

On various grounds the Italian physicist concludes that "many liquids, and especially those of Plateau's third category, are covered with a superficial pellicle; and that it is to this pellicle that they owe their great superficial viscosity." After the observations of Dupré* and myself,† supported as they are by the theory of Professor Willard Gibbs,‡ the existence of the superficial pellicle cannot be doubted; and its mode of action is thus explained by Marangoni§:—"The surface of a liquid, covered by a pellicle, possesses two superficial tensions; the first, which is the weaker and in constant action, is due to the pellicle; the second is in the latent state, and comes into operation only when the pellicle is ruptured. Since the latter tension exceeds the former, it follows that any force which tends to rupture the superficial pellicle upon a liquid encounters a resistance which increases with the difference of tensions between the liquid and the pellicle." In Plateau's experiment the advancing edge of the needle tends to concentrate the superficial contamination, and the retreating edge to attenuate it; the tension in front is thus inferior to the tension behind, and a force is called into operation tending to check the vibration. On a pure surface it is evident that nothing of this sort can occur, unless it be in a very subordinate degree, as the result of difference of temperature.

There is an important distinction, discussed by Willard Gibbs, according as the contamination, to which is due the lowering of tension, is merely accidentally present upon the surface, or is derived from the body of the liquid under the normal operation of chemical and capillary forces. In the latter case, that, for example, of solutions of soap and of camphor, the changes of tension which follow an extension or contraction of the surface may be of very brief duration. After a time, dependent largely upon the amount of contaminating substance present in the body of the liquid, equilibrium is restored, and the normal tension is recovered. On the other hand, in the case of a surface of water contaminated with a film of

* 'Théorie Mécanique de la Chaleur,' Paris, 1869, p. 377.

† "On the Tension of Recently Formed Liquid Surfaces," 'Roy. Soc. Proc.,' vol. 47, 1890, p. 281 (*supra*).

‡ 'Connecticut Acad. Trans.,' vol. 3, Part II, 1877-78. In my former communication I overlooked Prof. Gibbs's very valuable discussion on this subject.

§ 'Nuovo Cimento,' vol. 5-6, 1871-72, p. 260 (May, 1872).

insoluble grease, the changes of tension which accompany changes of area are of a permanent character.

It is not perfectly clear how far Marangoni regarded his principle of surface elasticity as applicable to the explanation of Plateau's observations upon distilled water; but, at any rate, he applied it to the analogous problem of the effect of oil in calming ripples. It is unfortunate that this attempt at the solution of a long-standing riddle cannot be regarded as successful. He treats the surface of the sea in its normal condition as contaminated, and therefore elastic, and he supposes that, upon an elastic surface, the wind will operate efficiently. When oil is scattered upon the sea, a non-elastic surface of oil is substituted for the elastic surface of the sea, and upon this the wind acts too locally to generate waves. It is doubtless true that an excess of oil may render a water surface again inelastic; but I conceive that the real explanation of the phenomenon is to be found by a precisely opposite application of Marangoni's principle, as in the theories of Reynolds* and Aitken.† Marangoni was, perhaps, insufficiently alive to the importance of *varying degrees* of contamination. An ordinary water surface is indeed more or less contaminated; and on that account is the less, and not the more, easily agitated by wind. The effect of a special oiling is, in general, to increase the contamination and the elasticity dependent thereupon, and stops short of the point at which, on account of saturation, elasticity would again disappear. The more elastic surface refuses to submit itself to the local variations of area required for the transmission of waves in a normal manner. It behaves rather as a flexible but inextensible membrane would do, and, by its drag upon the water underneath, hampers the free production and propagation of waves.

The question whether the effects observed by Plateau upon the surface of distilled water are, or are not, due to contamination must, I suppose, be regarded as still undecided. Oberbeck, who has experimented on the lines of Plateau, thus sums up his discussion:—"Wir müssen daher schliessen, entweder, dass der freien Wasseroberfläche ein recht bedeutender Oberflächenwiderstand zukommt, oder dass eine reine Wasseroberfläche in Berührung mit der Luft überhaupt nicht existirt."‡

Postponing for the moment the question of the origin of "superficial viscosity," let us consider its character. A liquid surface is capable of two kinds of deformation, dilatation (positive or negative) and shearing; and the question at once presents itself, is it the former or the latter which evokes the special resistance? Towards

* 'Brit. Assoc. Rep.,' 1880.

† 'Edinburgh Roy. Soc. Proc.,' 1882-83, vol. 12, p. 56.

‡ Wiedemann's 'Annalen,' vol. 11, 1880, p. 650.

the answer of this question Marangoni himself made an important contribution in the earlier of the memoirs cited. He found (p. 245) that the substitution for the elongated needle of Plateau of a circular disc of thin brass turning upon its centre almost obliterated the distinction between liquids of the two first categories. The ratio of the superficial to the internal viscosity was now even greater for ether than for water. From this we may infer that the special superficial viscosity of water is not called into play by the motions of the surface due to the rotation of the disc, which are obviously of the nature of shearing.

A varied form of this experiment is still more significant. I have reduced the metal in contact with the water surface to a simple (2″) ring ACBD of thin brass wire (fig. 1). This is supported by a fine

Fig 1.

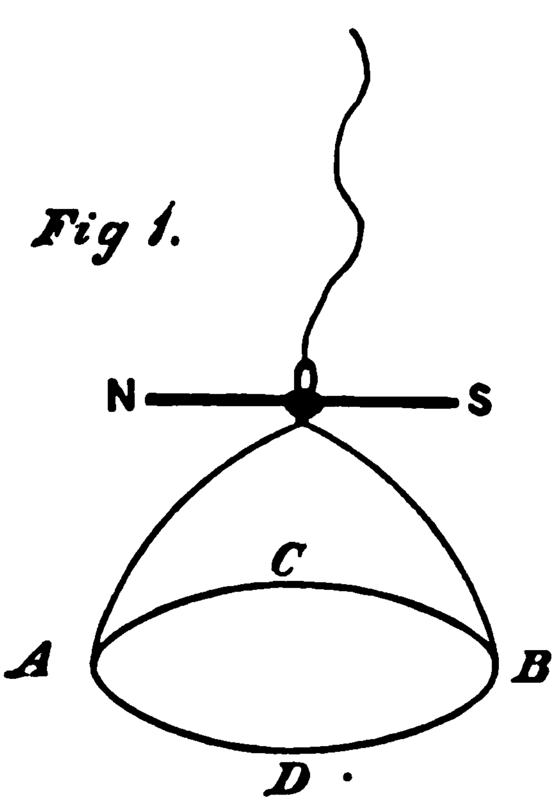
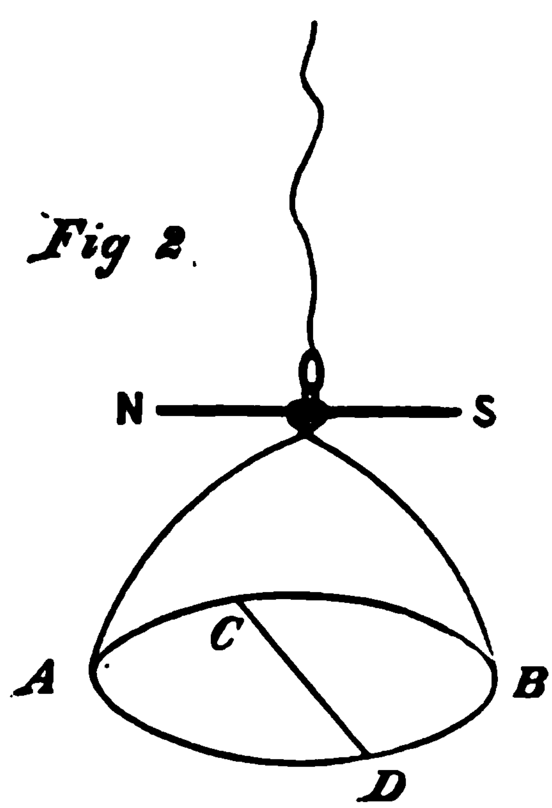


Fig 2.



silk fibre, so that it may turn freely about its centre. To give a definite set, and to facilitate forced displacements, a magnetised sewing needle, NS, is attached with the aid of wax. In order to make an experiment, the ring is adjusted to the surface of water contained in a shallow vessel. When all is at rest, the surface is dusted over with a little fine sulphur,* and the suspended system is suddenly set into rotation by an external magnet. The result is very distinct, and contrasts strongly with that observed by Plateau. Instead of the surface enclosed by the ring being carried round with it in its rotation, not the smallest movement can be perceived, except perhaps in the immediate neighbourhood of the wire itself. It is clear that an ordinary water surface does not appreciably resist shearing.

* Sulphur seems to be on the whole the best material, although it certainly communicates some impurity to the surface. Freshly heated pumice or wood-ashes sink immediately; and probably all powders really free from grease would behave in like manner.

A very slight modification of the apparatus restores the similarity to that of Plateau. This consists merely in the addition to the ring of a material diameter of the same brass wire, CD, fig. 2. If the experiment be repeated, the sulphur indicates that the whole water surface included within the semicircles now shares in the motion. In general terms the surface may be said to be carried round with the ring, although the motion is not that of a rigid body.

Experiments of this kind prove that what a water surface resists is not shearing, but local expansions and contractions of area, even under the condition that the total area shall remain unchanged. And this is precisely what should be expected, if the cause of the viscosity were a surface contamination. A shearing movement does not introduce any variation in the density of the contamination, and therefore does not bring Marangoni's principle into play. Under these circumstances there is no resistance.

It remains to consider liquids of the third category in Plateau's nomenclature. The addition of a little oleate of soda does not alter the behaviour of water, at least if the surface be tolerably fresh. On the other hand, a very small quantity of saponine suffices to render the surface almost rigid. In the experiment with the simple ring the whole interior surface is carried round as if rigidly attached. A similar effect is produced by gelatine, though in a less marked degree.

In the case of saponine, therefore, it must be fully admitted that there is a superficial viscosity not to be accounted for on Marangoni's principle by the tendency of contamination to spread itself uniformly. It seems not improbable that the pellicle formed upon the surface may have the properties of a solid, rather than of a liquid. However, this may be, the fact is certain that a contracting saponine surface has no definite tension alike in all directions. A sufficient proof is to be found in the well known experiment in which a saponine bubble becomes wrinkled when the internal air is removed.

The quasi-solid pellicle on the surface of saponine would be of extreme thinness, and, even if it exist, could hardly be recognisable by ordinary methods of examination. It would moreover be capable of re-absorption into the body of liquid if unduly concentrated by contraction of surface, differing in this respect from the gross, and undoubtedly solid, pellicles which form on the surface of hard water on exposure to the atmosphere.

Two further observations relative to saponine may here find a place. The wrinkling of a bubble when the contained gas is exhausted occurs also in an atmosphere (of coal gas) from which oxygen and carbonic acid are excluded.

In Plateau's experiment a needle which is held stiffly upon the surface of a saponine solution is to a great extent released when the

surface is contaminated by grease from the finger or by a minute drop of petroleum.

To return to the case of water, it is a question of the utmost importance to decide whether the superficial viscosity of even distilled water is, or is not, due to contamination with a film of foreign matter capable of lowering the tension. The experiments of Oberbeck would appear to render the former alternative very improbable; but, on the other hand, if the existence of the film be once admitted, the observed facts can be very readily explained. The question is thus reduced to this: Can we believe that the water surface in Plateau's apparatus is almost of necessity contaminated with a greasy film? The argument which originally weighed most with me, in favour of the affirmative answer is derived from the experiments of Quincke upon mercury. It is known that, contrary to all analogy, a drop of water does not ordinarily spread upon the surface of mercury. This is certainly due to contamination with a greasy film; for Professor Quincke* found that it was possible so to prepare mercury that water would spread upon it. But the precautions required are so elaborate that probably no one outside Professor Quincke's laboratory has ever witnessed what must nevertheless be regarded as the normal behaviour of these two bodies in presence of one another. The bearing of this upon the question under discussion is obvious. If it be so difficult to obtain a mercury surface which shall stand one test of purity, why may it not be equally difficult to prepare a water surface competent to pass another?

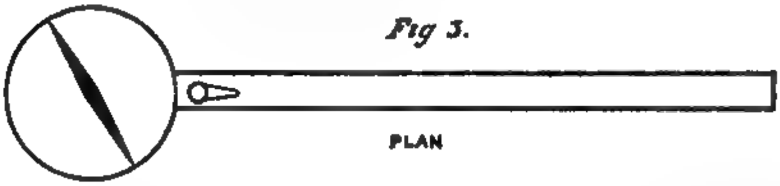
The method by which I have succeeded in proving that Plateau's superficial viscosity is really due to contamination consists in the preparation of a pure surface exhibiting quite different phenomena; and it was suggested to me by an experiment of Mr. Aitken.† This observer found that, if a gentle stream of air be directed vertically downwards upon the surface of water dusted over with fine powder, a place is cleared round the point of impact. It may be added that on the cessation of the wind the dust returns, showing that the tension of the bared spot exceeds that of the surrounding surface.

The apparatus, shown in figs. 3, 4, is constructed of sheet brass. The circular part, which may be called the *well*, has the dimensions given by Plateau. The diameter is 11 cm., and the depth 6 cm. The needle is 10 cm. long, 7 mm. in breadth at the centre, and about 0·3 mm. thick. It is suspended at a height of 2½ cm. above the bottom of the vessel. So far there is nothing special; but in connexion with the well there is a rectangular trough, or tail-piece, about 2½ cm. broad and 20 cm. long. Between the two parts a sliding door may

* Poggendorff's 'Annalen,' vol. 139, 1870, p. 66.

† *Loc. cit.*, p. 69.

Fig 3.



PLAN

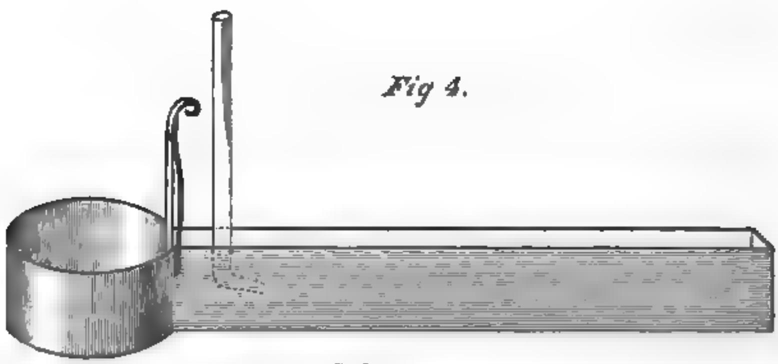


Fig 4.

Scale = $\frac{1}{4}$.

be inserted, by which the connexion is cut off, and the circular periphery of the well completed. The action of the apparatus depends upon a stream of wind, supplied from an acoustic bellows, and discharged from a glass nozzle, in a direction slightly downwards, so as to strike the water surface in the tail-piece at a point a little beyond the door. The effect of the wind is to carry any greasy film towards the far end, and thus to purify the near end of the tail-piece. When the door is up, this effect influences also the water surface in the well upon which the jet does not operate directly. For, if the tension there be sensibly less than that of the neighbouring surface in the tail-piece, an outward flow is generated, and persists as long as the difference of tensions is sensible. The movements of the surface are easily watched if a little sulphur be dusted over; when the water in the well has been so far cleansed that but little further movement is visible, the experiment may be repeated without changing the water by contaminating the surface with a little grease from the finger or otherwise. In this way the surface may be freed from an insoluble contamination any number of times, the accumulation of impurity at the far end of the tail-piece not interfering with the cleanness of the surface in the well.

Another device that I have usually employed facilitates, or at any rate hastens, the cleansing process. When the operation is nearly complete, the movement of the surface becomes sluggish on account of the approximate balance of tensions. At this stage the movement

may be revived, and the purification accelerated, by the application of heat to the bottom of the well at the part furthest removed from the tail-piece. It may, perhaps, be thought that convection currents might be substituted altogether for wind; but in my experience it is not so. Until a high degree of purity is attained, the operation of convection currents does not extend to the surface, being resisted by the film according to Marangoni's principle.

When the apparatus was designed, it was hoped that the door could be made a sufficiently good fit to prevent the return of the greasy film into the well; but experience showed that this could not be relied upon. It was thus necessary to maintain the wind during the whole time of observation. The door was, however, useful in intercepting mechanical disturbance.

A very large number of consistent observations have been recorded. The return of the needle, after deflection to 90°, is timed over an arc of 60°, viz., from 90° to 30°, and is assisted by a fixed steel magnet acting in aid of the earth's magnetism. A metronome, beating three times per second, facilitates the time measurement. An an example, I may quote some observations made on April 11.

The apparatus was rinsed and carefully filled with distilled water. In this state the time was 12 (beats). After blowing for a while there was a reduction to 10, and after another operation to 8. The assistance of convection currents was then appealed to, and the time fell to $6\frac{3}{4}$, and after another operation to 6. This appeared to be the limit. The door was then opened, and the wind stopped, with the result that the time rose again to 12. More water was then poured in until the needle was drowned to the depth of about half an inch. Under these conditions the time was $6\frac{3}{4}$.

It will be seen, that while upon the unprepared surface the time was nearly twice as great as in the interior, upon the purified surface the time was somewhat less than in the interior.

For the sake of comparison, precisely similar observations were made upon the same day with substitution for water of methylated alcohol. Before the operation of wind the time was 5; after wind, 5; on repetition, still 5. Nor with the aid of convection currents could any reduction be effected. When the needle was drowned, the time rose to $7\frac{1}{4}$. The alcohol thus presents, as Plateau found, a great contrast with the unprepared water; but comparatively little with the water after treatment by wind and heat.

An even more delicate test than the time of vibration is afforded by the behaviour of the surface of the liquid towards the advancing edge of the needle. In order to observe this, it is necessary to have recourse to motes, but all superfluity should be avoided. In a good light it is often possible to see a few motes without any special dusting over. In my experience, an unprepared water surface always

behaves in the manner described by Plateau; that is, it takes part in rotation of the needle, almost from the first moment. Under the action of wind a progressive change is observed. After a time the motes do not begin their movement until the needle has described a considerable arc. At the last stages of purification, a mote, situated upon a radius distant 30° or 40° from the initial direction of the needle, retains its position almost until struck; behaving, in fact, exactly as Plateau describes for the case of alcohol. I fancied, however, that I could detect a slight difference between alcohol and water even in the best condition, in favour of the former. With a little experience it was easy to predict the "time" from observations upon motes; and it appeared that the last degrees of purification told more upon the behaviour of the motes than upon the time of describing the arc of 60°. It is possible, however, that a different range from that adopted might have proved more favourable in this respect.

The special difficulties under which Plateau experimented are well known, and appealed strongly to the sympathies of his fellow workers; but it is not necessary to refer to them in order to explain the fact that the water surfaces that he employed were invariably contaminated. Guided by a knowledge of the facts, I have several times endeavoured to obtain a clean surface without the aid of wind, but have never seen the time less than 10. More often it is 12, 13, or 14. It is difficult to decide upon the source of the contamination. If we suppose that the greasy matter is dissolved, or, at any rate, suspended in the body of the liquid in a fine state of subdivision, it is rather difficult to understand the comparative permanence of the cleansed surfaces. In the case of distilled water, the condition will usually remain without material change for several minutes. On the other hand, with tap water (from an open cistern), which I have often used, although there is no difficulty in getting a clean surface, there is usually a more rapid deterioration on standing. The progressive diminution of the tension of well-protected water surfaces observed by Quincke* is most readily explained by the gradual formation of a greasy layer composed of matter supplied from the interior, and present only in minute quantity; although this view did not apparently commend itself to Quincke himself. If we reject the supposition that the greasy layer is evolved from the interior of the liquid, we must admit that the originally clean free surface, formed as the liquid issues from a tap, is practically certain to receive contamination from the solid bodies with which it comes into contact. The view, put forward hypothetically by Oberbeck, that contamination is almost instantly received from the atmosphere is inconsistent with the facts already mentioned.

Some further observations, made in the hope of elucidating this

* Poggendorff's 'Annalen,' vol. 160, 1877, p. 580.

question, may here be recorded. First, as to the effect of soap, or rather oleate of soda. A surface of distilled water was prepared by wind and heat until the time was $5\frac{1}{2}$, indicating a high degree of purity. The door being closed, so as to isolate the two parts of the surface, and the wind being maintained all the while, a few drops of solution of oleate were added to the water in the tail-piece. With the aid of gentle stirring, the oleate found its way, in a few minutes, under the door, and reached the surface of the water in the well. The time gradually rose to 13, 14, 15; and no subsequent treatment with wind and heat would reduce it again below 12. In this case there can be no doubt that the contamination comes from the interior, and is quickly renewed if necessary; not, however, so quickly that the tension is constant in spite of extension or the surface would be free from superficial viscosity.

In like manner, the time upon the surface of camphorated distilled water could not be reduced below 10, and the behaviour of motes before the advancing needle was quite different to that observed upon a clean surface. A nearly saturated solution of chloride of sodium could not be freed from superficial viscosity; while, on the other hand, an addition of $\frac{1}{3}$ per cent. of alcohol did not modify the behaviour of distilled water.

The films of grease that may be made evident in Plateau's apparatus are attenuated in the highest degree. In a recent paper* I have estimated the thickness of films of olive oil competent to check the movements of camphor fragments as from one to two micro-millimetres; but these films are comparatively coarse. For example, there was never any difficulty in obtaining from tap-water surfaces upon which camphor was fully active without the aid of wind or special arrangements. I was naturally desirous of instituting a comparison between the quantities necessary to check camphor movements and the more minute ones which could be rendered manifest by Plateau's needle; but the problem is of no ordinary difficulty. A direct weighing of the contamination is out of the question, seeing that the quantity of oil required in the well of the apparatus, even to stop camphor, would be only $\frac{1}{64}$ milligram.

The method that I have employed depends upon the preparation of an ethereal solution of olive oil, with which clean platinum surfaces are contaminated. It may be applied in two ways. Either we may rely upon the composition of the solution to calculate the weight of oil remaining upon the platinum after evaporation of the solvent, or we may determine the relative quantities of solution required to produce the two sorts of effects. In the latter case we are independent of the precise composition of the solution, and more especially of the

* *Supra*, p. 364.

question whether the ether may be regarded as originally free from dissolved oil of an involatile character. In practice, both methods have been used.

The results were not quite so regular as had been hoped, the difficulty appearing to be that the oil left by evaporation upon platinum was not completely transferred to the water surface when the platinum was immersed, even although the operation was performed slowly, and repeated two or three times. On the other hand, there was no difficulty in cleansing a large surface of platinum by ignition in the flame of a spirit lamp, so that it was absolutely without perceptible effect upon the movement of the needle over a purified water surface.

The first solution that was used contained 7 milligrams of oil in 50 c.c. of ether. The quantities of solution employed were reckoned in drops, taken under conditions favourable to uniformity, and of such dimensions that 100 drops measured 0·6 c.c. The following is an example of the results obtained :—On April 25, the apparatus was rinsed out and recharged with distilled water. Time = 13. After purification of surface by wind and heat, $5\frac{1}{2}$, rising, after a considerable interval, to 6. After insertion of a large plate of platinum, recently heated to redness, time unchanged. A narrow strip of platinum, upon which, after a previous ignition, three drops of the ethereal solution had been evaporated, was then immersed, with the result that the time was at once increased to $8\frac{1}{2}$. In subsequent trials two drops never failed to produce a distinct effect. Special experiments, in which the standard ether was tested after evaporation upon platinum, showed that nearly the whole of the effect was due to the oil purposely dissolved.

The determination of the number of drops necessary to check the movements of camphor upon the same surface seemed to be subject to a greater irregularity. In some trials 20 drops sufficed; while in others 40 or 50 drops were barely enough. There seems to be no doubt that the oil is left in a rather unfavourable condition,* very different from that of the compact drop upon the small platinum surface of former experiments; and the appearance of the platinum on withdrawal from the water often indicates that it is still greasy. Under these circumstances it is clearly the smaller number that should be adopted; but we are safe in saying that $\frac{1}{10}$ of the oil required to check camphor produces a perceptible effect upon the time in Plateau's experiment, and still more upon the behaviour of the surface before the advancing needle, as tested by observation of motes. At this rate the thickness at which superficial viscosity

* It should be stated that the evaporation of the ether, and of the dew which was often visible, was facilitated by the application of a gentle warmth.

becomes sensible in Plateau's apparatus is about $\frac{1}{10}$ of a micro-millimetre, or about $\frac{1}{6000}$ of the wave-length of yellow light.

A tolerably concordant result is obtained from a direct estimate of the smaller quantity of oil, combined with the former results for camphor, which were arrived at under more favourable conditions. The amount of oil in two drops of the solution is about 0·0017 milligram. This is the quantity which suffices to produce a visible effect upon the needle. On the large surface of water of the former experiments the oil required to check camphor was about 1 milligram. In order to allow for the difference in area, this must be reduced 64 times, or to 0·016 milligram. According to this estimate the ratio of thicknesses for the two classes of effects is about as 10 : 1.

Very similar results were obtained from experiments with an ethereal solution of double strength, one drop of which, evaporated as before, upon platinum, produced a distinct effect upon the time occupied by the needle in traversing the arc from 90° to 30°.

I had expected to find a higher ratio than these observations bring out between the thicknesses required for the two effects. The ratio 15 : 1 does not give any too much room for the surfaces of ordinary tap water, such as were used in the bath observations upon camphor, between the purified surfaces on the one side and those oiled surfaces upon the other, which do not permit the camphor movements.

It thus became of interest to inquire in what proportion the film originally present upon the water in the bath experiments requires to be concentrated in order to check the motion of camphor fragments. This information may be obtained, somewhat roughly it is true, by dusting over a patch of the water surface in the centre of the bath. When a weighed drop of oil is deposited in the patch, it drives the dust nearly to the edge, and the width of the annulus is a measure of the original impurity of the surface. When the deposited oil is about sufficient to check the camphor movements, we may infer that the original film bears to the camphor standard a ratio equal to that of the area of the annulus to the whole area of the bath. Observations of this kind indicated that a concentration of about six times would convert the original film into one upon which camphor would not freely rotate.

Another method by which this problem may be attacked depends upon the use of flexible solid boundary. This was made of thin sheet brass, and is deposited upon the bath in its expanded condition, so as to enclose a considerable area. Upon this surface camphor rotates, but the movement may be stopped by the approximation of the walls of the boundary. The results obtained by this method were of the same order of magnitude.

If these conclusions may be relied upon, it will follow that the initial film upon the water in the bath experiments is not a large

multiple of that at which superficial viscosity tends to disappear. At the same time, the estimate of the total quantity of oil which must be placed upon a really pure surface in order to check the movements of camphor must be somewhat raised, say, from 1·6 to 1·9 micro-millimetre. It must be remembered, however, that on account of the want of definiteness in the effects, these estimates are necessarily somewhat vague. By a modification of Plateau's apparatus, or even in the manner of taking the observations, such as would increase the extent of surface from which the film might be accumulated before the advancing edge of the needle, it would doubtless be possible to render evident still more minute contaminations than that estimated above at one-tenth of a micro-millimetre.

[*Postscript, June* 4.—In order to interpret with safety the results obtained by Plateau, I thought it necessary to follow closely his experimental arrangements; but the leading features of the phenomenon may be well illustrated without any special apparatus. For this purpose, the needle of the former experiments may be mounted upon the surface of water contained to a depth of 1 or 2 inches in a large flat bath. Ordinary cleanliness being observed, the motes lying in the area swept over by the needle are found to behave much as described by Plateau. Moreover, the motion of the needle under the action of the magnet used to displace it is decidedly sluggish. In order to purify the surface, a hoop of thin sheet brass is placed in the bath, so as to isolate a part including the needle. The width of the hoop must of course exceed the depth of the water, and that to an extent sufficient to allow of manipulation without contact of the fingers with the water. If the hoop be deposited in its contracted state, and be then opened out, the surface contamination is diminished in the ratio of the areas. By this simple device there is no difficulty in obtaining a highly purified surface, upon which motes lie quiescent, almost until struck by the oscillating needle. In agreement with what has been stated above, an expansion of three or four times usually sufficed to convert the ordinary water surface into one upon which superficial viscosity was tending to disappear.

I propose to make determinations of the actual tensions of surfaces contaminated to various degrees; but in the meantime it is evident that the higher degrees of purity do not imply much change of tension. In the last experiment upon a tolerably pure surface, if we cause the needle to oscillate rapidly backwards and forwards through a somewhat large angle, we can clear away the contamination from a certain area. This contamination will of course tend to return, but observation of motes shows that the process is a rather slow one.

The smallness of the forces at work must be the explanation of the failure to clean the surface in Plateau's apparatus by mere expansion.

For this experiment the end wall was removed from the tail-piece (fig. 3), and a large flexible hoop substituted. By this means, it was hoped that when the whole was placed in the bath it would be possible, by mere expansion of the hoop, to obtain a clean surface in the well. The event proved, however, that the purification did not proceed readily beyond the earlier stages, unless the passage of the contamination through the long channel of the tail-piece was facilitated by wind.]

V. "Experiments with Lord Rayleigh's Colour Box." By ARTHUR SCHUSTER, F.R.S. Received May 15, 1890.

Lord Rayleigh described before the meeting of the British Association, in 1881,* a colour box in which artificial yellow is produced by mixing a pure red and green, and this yellow is directly compared to the yellow of the spectrum. Lord Rayleigh has given an account of certain peculiarities of vision observed in a number of persons, and it seemed to me worth while to extend the enquiry to a greater number of observers, and also, if possible, to obtain some evidence as to the existence of smaller differences than those described in Lord Rayleigh's paper.

The instrument used was made according to Lord Rayleigh's second pattern, in which a double-image prism is interposed between the slit and collimator lens; the prism which separates the light being a direct-vision prism. For the detailed description of the instrument I must refer the reader to Lord Rayleigh's paper.

My attention was in the first instance directed to prove or disprove the existence of small differences in different persons. It was necessary, therefore, only to take persons in whose observing powers I could place some reliance, and, secondly, to multiply the number of observations of each individual, so as to obtain an idea of the degree of accuracy to which the observations could be trusted. The instrument was used in a fairly dark room, and the observer was asked to place the Nicol so as to obtain the required match. After the reading had been taken the Nicol was displaced and the observations repeated. Five separate readings were thus generally obtained, and occasionally more. Often separate sets of observations were made for the right and left eye. As, owing to imperfections of construction, the zero of the instrument did not remain constant, either myself or Mr. Hadley, one of the demonstrators in the Phyisical Laboratory, took a reading whenever observations were made.

I have often compared my vision with Mr. Hadley's, and never detected any difference amounting to more than 0·1 of a division of the

* 'Nature,' Nov. 17, 18'1.

scale of my instrument; so that I have taken our colour matches as equal, and referred all others to them, although, as will appear, we do not quite agree with the majority of people. In order to understand the numbers given in the following pages, it is necessary to state that in the neighbourhood of normal vision a difference of one-tenth of a division means a difference of about $2\frac{1}{2}$ per cent. in the ratio of intensities of red to green. I consider that the mean of five readings of a practised observer should not differ by more than that on different occasions. If two good observers place the Nicol half a division different from each other, then each should be able in his own mind to be certain that the other's match is wrong. The difference of a whole division is generally very obvious after a little practice with the instrument.

It appears that differences of between a half and whole division are very common, so that there cannot be any doubt of the real existence of small differences, possibly following, as far as the number of observations allow us to say, the ordinary law for deviations from a mean. But the larger differences, such as Lord Rayleigh was the first to observe, seem certainly to be more frequent than the distribution of small differences would lead us to expect. As has already been stated, it seemed better in the first instance to confine myself to a careful examination of a limited number of cases than to extend the enquiry too much before I could form an estimate of the accuracy which is to be expected from a casual observer. I have examined seventy-five: of these three proved colour blind; four, of which three belonged to the same family, showed the same peculiarity of vision as Mr. Balfour and Professor J. J. Thomson, while one showed a large difference in the opposite direction.

In the following tables I shall call my own reading zero, and shall take as the *unit* difference between myself and others *one-tenth of a division* of the divided circle. The first table gives the ratio of red to green used in the match, corresponding to the stated differences of reading:—

Table I.

−70	6·15	+ 2	0·82
−60	4·37	+ 4	0·78
−50	3·21	+ 6	0·74
−40	2·41	+ 8	0·70
−30	1·84	+10	0·67
−20	1·42	+20	0·51
−10	1·11	+30	0·39
−8	1·05	+40	0·29
−6	1·00	+50	0·21
−4	0·95	+60	0·15
−2	0·90	+70	0·10
±0	0·86		

It will be seen from this table that the proportion of red to green which I require to make an artificial yellow is 0·86. This number is not comparable with those given by Lord Rayleigh for his own and Mr. Balfour's vision, as everything depends on the particular green which he uses to make the match. I was aiming in setting up the instrument to choose such a red and green as would without the Nicol make a yellow, and perhaps took too much of a yellowish-green in consequence.* Table II exhibits the results obtained for various observers. The means only of all observations taken at different times, and with both eyes, are given.

Table II.

Observer.	Number of readings.	Mean of readings.	Mean difference of readings.
1	6	0	1·0
2	17	−1·0	2·3
3	10	+1·5	5·3
4	25	0	1·9
6	12	−4·0	4·4
7	5	+3·0	3·4
8	5	−1·0	2·8
9	20	−4·0	1·8
10	5	−7·0	2·2
11	5	+2·0	3·6
12	7	+3·3	2·0
13	5	0	1·0
14	15	−7·3	2·3
15	15	+3·0	2·7
16	6	−11·5	0·9
18	5	−5·0	3·6
19	5	−5·0	4·0
20	5	−12·0	2·0
21	20	−7·2	3·1
22	5	−4·0	2·8
23	12	−1·5	4·3
24	5	−11·0	1·6
25	10	+3·5	2·9
26	20	−5·3	1·8
27	20	0	1·3
29	18	+1·2	2·0

* [Note, added May 23.—Since writing the above, I have determined the wave-lengths of the colours selected, and was surprised to find how much more yellow the green was than I thought or intended; the wave-length of the green which was mixed with the red was about $5·620 \times 10^{-5}$. The red was a full red near C, the yellow about the sodium yellow.]

Table II—*continued.*

Observer.	Number of readings.	Mean of readings.	Mean difference of readings.
30	23	−5·0	1·4
31	14	−9·0	2·6
32	10	−2·5	1·8
33	8	−2·0	1·6
34	9	−1·5	2·3
35	10	−3·0	2·4
36	10	−15·0	1·2
37	20	−5·5	2·1
38	10	−3·5	1·8
39	10	0	2·6
40	10	−10·0	2·1
41	20	9·5	2·1
42	26	−6·7	2·4
43	10	−2·0	1·1
44	10	−14·0	2·2
45	20	−2·2	2·2
47	10	−11·0	3·0
48	10	−5·5	2·7
49	10	−2·0	1·8
51	10	−1·5	2·8
52	10	−6·5	5·0
53	10	−2·5	5·0
54	10	−3·0	1·5
55	10	−8·5	3·5
56	10	−4·5	2·8
57	10	−1·0	7·5
58	10	−16·0	3·4
59	10	−13·5	2·5
60	10	−14·0	3·2
61	5	−11·0	0·8
62	5	+3·0	2·0
63	8	−1·0	1·7
64	5	−6·0	1·2
65	5	−4·0	2·8
67	15	−4·0	2·8
68	5	−9·0	2·8
69	5	−2·0	3·0
71	10	−4·0	2·9
73	10	+1·0	1·1
74	10	−3·0	3·0
75	6	−5·0	

Table II—*continued.*

Observer.	Number of readings.	Mean of readings.	Mean difference of readings.
5	20	+56·0	5·8
17	5	+33·0	3·0
28	28	−44·0	3·3
50	10	+71·0	7·4
72	10	+43·0	2·5

46 Colour blind.
66 Colour blind.
70 Colour blind.

The first column in the above table gives the number which identifies the observer, the second column the number of readings taken, the third the mean difference between the observer and myself, or Mr. Hadley. The fourth column gives an idea of the consistency of the same observer in reading. It is the mean difference between a single reading and the mean of all the readings, negative differences being counted as positive ones. A glance at the table shows that this mean difference is generally about two or three tenths of a division.

From the results of column 3, Table III has been obtained. Column II gives the number of observers whose readings do not differ by more than half a unit from the numbers given in column I. Column III gives the mean of three successive values of column II, that row against which the number is placed being the middle of the numbers whose mean is taken.

Table III.

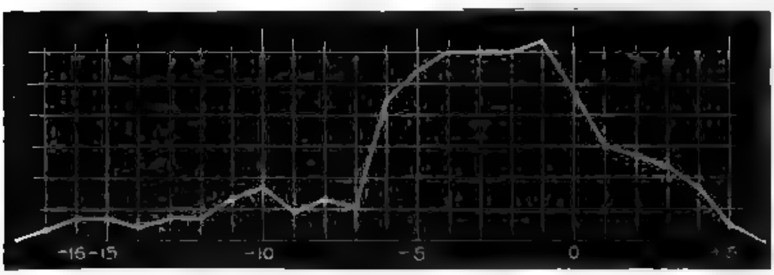
Column I.	II.	III.
−16	1	0·7
−15	1	1·7
−14	3	1·3
−13	0	1·7
−12	2	1·7
−11	3	2·3
−10	2	2·7
−9	3	1·7
−8	0	2·3
−7	4	2·0
−6	2	4·3
−5	7	5·7
−4	7	6·0

Table III—*continued.*

Column I.	II.	III.
−3	4	6·0
−2	7	6·0
−1	7	6·3
0	5	4·7
+1	2	3·0
+2	2	2·7
+3	4	2·3
+4	1	1·7

The results of this table, in which the few observers showing large differences are not included, is plotted in fig. 1, in which the numbers in column I of the above table are taken as ordinates, and the numbers in column III as abscissæ.

Fig. 1.



We see at once that the greatest number of observers read between −1 and −4. The curve falls rapidly on either side, but there are more observers within the limits of the table apparently showing large negative than large positive differences. Thus, for instance, taking 3 as the mean value of all observers, there was nobody amongst sixty-seven observers differing by eight-tenths or more of a division at the positive, while there were as many as ten differing by the same amount on the negative side. If we were from the given curve to calculate the probability of such large differences, as shown by the five observers, 5, 17, 28, 50, and 72, we should get exceedingly small numbers; this confirms Lord Rayleigh's statement, that differences from normal vision do not seem to follow the law of errors. For differences less than one division of the scale the curve is not unlike the curve of errors, but not for the larger differences; thus half the total number of observers read within 3·5 units of the average. If the difference from normal sight was to follow the law of

errors, we should only have one observer in 50,000 who would read twenty-one units different from the rest. While counting the different members of the same family only as one, I have found three such large differences among seventy-five, and Lord Rayleigh found the same number among thirty observers. He also examined seven female observers, none showing any decided difference from the mean. In the above table numbers 9, 23, 65, and 74 are women, and their readings −4, −1·5, −4, −3, are very consistent with each other; on the other hand, it must be noted, as a remarkable exception, that 72 is also a woman; her husband has normal sight, but amongst three sons two, viz., 5 and 17, show the same peculiarity.

It is instructive to compare the readings of numbers 28 and 50 with the ratios of intensities given in Table I. It will be seen that, while 28 requires about 2·8 times as much red as green to make yellow, 50 requires nearly five times as much *green* as red to produce yellow. That the ratio of red to green required by one observer is thirteen times as great as that required by the other. How different will compound colours look to these observers! It seems remarkable, however, that both agree in the particular wave-length which they call yellow, and the actual sensation of *pure* colours seems therefore to be the same for both. It seems difficult to explain this fact in any other way than that suggested by Professor Maxwell to account for some peculiarities of his own eyesight, namely, by a selective absorption in the yellow spot of the eye, which differs in different individuals. To judge from the diagram given in this paper, Maxwell had, as compared with his wife, the same peculiarity of eyesight as the different observers mentioned by Lord Rayleigh and number 5 above; this could be explained by greater absorption of green in the yellow spot. But, further, Maxwell's eyes presented an opposite peculiarity for the rays between the green and violet, he wanting *less* green to produce blue than Mrs. Maxwell. This Maxwell tries to explain by more pronounced absorption of the blue rays than of the green. I cannot quite follow him in this explanation, because the greater absorption of blue does not seem to me to affect its position on the colour diagram, but only the intensity of the mixture produced by green and violet. I can only account for this second peculiarity of Maxwell's, that in his case the absorption of the *violet* primary colour was stronger than that of the green. If we adopt the hypothesis that the different position of the pure colours in the colour diagram of different observers is due to an absorption of light in the media of the eye before it reaches the retina, we are at liberty to assume that the sensation of yellow in all eyes is due to an excitation of nerves sensitive to green and red respectively in a fixed proportion.

Direct experiments to determine the absorption in the yellow spot

of the eye have been made by Glan, and it seems to me of importance to repeat and extend his observations. One point, especially, is worth clearing up: in how far are the complementary colours the same for different eyes? As far as I can judge, according to the view just explained, they should be the same.*

I have paid some attention to the possibility of a change in the reading of the same observer at different times; but it is very difficult to obtain decisive evidence in this respect. It is a curious fact, however, that the difference in the reading of the same observer at different times will differ more from each other than one would be led to expect from the consistency of his differing readings taken the same day. As this happens, however, chiefly with observers whose accuracy I have reason to doubt on other grounds, little value can be attached to such differences. It is possible that a careless observer remembers from observation to observation on the same day what he has called a match, though it may be a trifle too green or too red, and in this way the readings may gain an appearance of too great consistency. I take, for instance, number 42, whose readings are characteristic in this respect.

Date.	Number of observations.	Mean reading.	Average difference between each reading and mean reading.
Dec. 4, 1889........	5	0	2·0
Feb. 13, 1890.......	5	−11	1·9
April 23, 1890.... .	8	−6	2·6
May 6, 1890........	8	−10	3·1

On December 4, amongst five readings, he never read lower than −4; while on February 13, he never read *higher* that −4. If I had reason to believe number 42 a careful observer, I should take this as a proof of a change in his eyesight; I am afraid, however, no certain conclusions can be drawn from his observations. But there are some other cases of marked differences.

I have already stated that I have constantly compared my sight with that of Mr. Hadley without being able to trace any decided difference.

The following are a few examples of readings taken at different times with observers in whose judgment I can place reliance:—

* [This is not quite correct, partly owing to the indefinite nature of what we call white. What I meant to say is this: If six pure colours, p, q, r, s, t, u, are related to each other by the equations $ap + bq = cr + ds = et + fu$, then a second observer whose eyes only differed by a different absorption in their media should be able to match the six colours so as to obtain the equations $a'p + b'q = c'r + d's = e't + f'u$. If the resulting colour for the normal eye is white p and q, &c., are complementary colours, but the resulting colour for the second observer would not necessarily appear white to him.—May 23.]

Number 2—	
March 4, 1889	−2
Jan. 20, 1890	0
April 23, 1890	−1
Number 4—	
March 5, 1889	+5
May 6, 1890	−3
Number 9—	
March 6, 1889	−4
March 20, 1889	−3
April 25, 1890	−2
May 7, 1890	−7
Number 14—	
March 6, 1889	−7
May 5, 1890	−6
Number 15—	
March 6, 1889	+1
May 6, 1890	+3
Number 21—	
March 20, 1889	−6
May 2, 1890	−8
Number 25—	
March 21, 1889	+4
May 6, 1890	+3
Number 26—	
March 24, 1889	−2
May 6, 1890	−7

Most of these observations show no change, and are indeed remarkably consistent. I must except, however, number 4, who is a very trustworthy observer; and number 26 (Professor Dixon). In the latter case, a change seems almost certain either in his eye or in mine. We both observe, however, as Clerk Maxwell has done, that the matches are not quite the same according as one looks straight at the coloured patches, or a little to one side; this would support the view that the absorption in the yellow spot plays an important part.

There is no evidence as to a difference in reading between the right and left eye, except in one case. Generally speaking, the reading

taken with two eyes agrees very well. In the case of one observer, number 30, although no difference could be traced on November 27, 1889, the difference was half a division on May 7, 1890, and sufficient for him to be satisfied that when he made a match with one eye it did not appear a match with the other.

Presents, June 5, 1890.

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Revue Médico-Pharmaceutique. Année 3. No. 4. 4to. *Constantinople* 1890. The Editor.

Stazioni Sperimentali Agrarie Italiane. Vol. XVIII. Fasc. 4. 8vo. *Asti* 1890. R. Stazione Enologica, Asti.

Thirty-six *Carte de Visite* Photographs of Fellows of the Royal Society. Messrs. Maull and Fox.

June 12, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

Sir Benjamin Baker, Mr. R. H. M. Bosanquet, Mr. S. H. Burbury, Mr. W. Gardiner, Dr. A. S. Lea, Major P. A. MacMahon, Professor S. U. Pickering, Mr. I. Roberts, Mr. J. J. H. Teall, and Dr. R. T. Thorne were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a Re-determination of the principal Line in the Spectrum of the Nebula in Orion, and on the Character of the Line." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received March 20, 1890.

[Publication deferred.]

II. "Note on the Photographic Spectrum of the Great Nebula in Orion." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received April 16, 1890.

[Publication deferred.]

III. "On a new Group of Lines in the Photographic Spectrum of Sirius." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received April 25, 1890.

[Publication deferred.]

IV. "Preliminary Note on the Development of the Tuatara (*Sphenodon punctatum*)." By Professor A. P. W. THOMAS, M.A., F.L.S., F.G.S., University College, Auckland, N.Z. Communicated by Professor E. RAY LANKESTER, F.R.S. Received May 19, 1890.

A grant was made by the Royal Society in the year 1884 to Professor J. T. Parker, of Dunedin, for the study of the development of *Apteryx*, *Sphenodon* and *Callorhynchus*. As *Sphenodon* does not occur near Dunedin, but is found chiefly on outlying islands belonging to the province of Auckland, at the opposite end of the colony, Professor Parker invited me to join him in the investigation of this form.

We gathered from what had been written on the natural history of the tuatara, as well as from oral information obtained from those who were best acquainted with the New Zealand fauna, that the month of February was probably the time at which the tuatara bred.

We therefore started from Auckland at the beginning of February, 1885, for what appeared the most promising hunting-ground—the island of Karewa, some ten miles from Tamanga, in the Bay of Plenty. Mr. A. Reischek, a naturalist well known by his researches on the natural history of New Zealand, who had already had opportunities of observing the tuatara, was good enough to accompany us.

The island of Karewa is situated some four miles from the mainland; it is little more than a rock which rises with precipitous sides high above the sea. At one spot only can a landing be effected, as the island is exposed to the ocean swell rolling in from the open Pacific, whilst the shores are for the greater part formed by unscalable cliffs of rhyolitic rock. Our first attempt at landing was frustrated by the swell, but a day later a second attempt was successful.

The vegetation on the island is largely composed of small karaka trees and thickets of coprosma. The light, loose soil between the roots of trees and the rocks is mined by countless burrows, in which live mutton-birds (*Puffinus tenuirostris*) and tuataras.

On the "Chicken Islands" the tuataras have been described by Mr. Reischek as living with certain other sea-birds, namely, another

species of *Puffinus* (*P. gavius*) and two species of *Procellaria*. This has been cited in 'Nature'* as an interesting case of commensalism, and it is there stated that the birds "live in holes dug out by the tuataras and keep apparently on the best terms with them." It is stated that the tuatara generally lives on the right and the sea-bird on the left of the inner chamber.

I believe it is quite a mistake to suppose that any friendly relation whatever exists between the tuataras and the birds, and that here, as in the somewhat similar case of the prairie dog and rattlesnake, the idea of friendliness is quite out of place. It is true that the tuatara sometimes makes use of the burrows of the mutton-bird (though I have never found a tuatara living in the same burrow as a mutton-bird), but it cannot be said that the two species ever live on more favourable terms than those of mutual toleration. At one time I kept two kiwis in a large house with a number of tuataras, and a tuatara would at times shelter itself in the same box or corner as a kiwi—the two never attempting to fight, the tuatara hiding itself under the kiwi as it would do under a stone, whilst the kiwi seemed not to notice its presence. The tuatara seems to enter the burrow of the mutton-bird just as it would shelter itself in any other hole in the ground.

In any case, the tuatara must be an untrustworthy associate, for on four occasions I have seen or captured tuataras with young mutton-birds in their mouths. It is probably a truer view of the situation to suppose that the chance of getting a nestling renders the burrows of the mutton-bird more attractive to the tuatara.

Dr. Günther, in the absence of personal observation, supposed that the tuatara was incapable of burrowing. It is, however, certain that it can burrow well in such light soil as is found on Karewa, and even in the clay soil on which my lizard-houses are situated the animals have made burrows fully two feet in length, in which they are completely hidden. Not all the lizards, however, are so industrious; most of them have contented themselves with the artificial burrows, in the shape of long wooden boxes and drain pipes, with which they have been provided.

We spent some days camping on the island and captured a number of tuataras, but searched in vain for eggs, though we opened up many burrows in the hope of finding them. The smallest tuatara found was 4·9 inches in length. Not finding eggs, we dissected several tuataras, thinking that the condition of the ovaries might tell us whether we were too early or too late in the breeding season, but the dissection revealed no eggs at all approaching maturity.

We took away from the island a number of tuataras, hoping that

* October 19, 1882. See also 'Transactions of the New Zealand Institute,' vol. 14, p. 274.

they would breed in captivity. Some of these were taken by Professor Parker to Dunedin, a rather larger number being kept by me in Auckland, as it was thought that the warmer climate of the northern part of the colony would be more favourable.

Up to the beginning of January, 1886, no eggs had been obtained from my tuataras, and, as regards the lizards I then possessed, any such hope was futile, for I shortly afterwards discovered that all my tuataras (twenty-nine in number) were males. Thinking that our visit the previous summer had been too late in the season, I determined to make another expedition to Karewa, this time at my own expense. Professor Parker was unable to join me, owing to the great distance of Dunedin from the spot; he was, moreover, engaged in working at the development of the kiwi. From this date, therefore, the whole of the work fell into my hands. I spent three or four days at the beginning of January in camp alone on Karewa; but, although a month earlier in the season than on the occasion of the previous visit, I was again unsuccessful in procuring a single egg of the tuatara.

I made, however, a step in advance by discovering the external differences between the sexes. We had been assured by those familiar with the tuatara that there was no difference in the external characters of the sexes, and this statement seemed to be borne out by what we could learn from the literature of the subject. Thus, Dr. Newman, the latest writer on the subject, said*: "The males are so like the females that they have not yet been distinguished with certainty."

"The male tuatara has no special strongly marked tints, no special personal attraction; and, unlike the males of several other species of lizards, are not much, if at all, bigger than the females. The absence of special sexual attributes is perhaps due . . ."

On dissecting and carefully comparing a number of tuataras, I found that the current statements were not correct. There need seldom be any difficulty in distinguishing the sexes; the male is much larger, and has the crests on neck and back far more strongly developed. In the fully adult male, the crests with their white spines are very conspicuous; in the female, the crests are low, and the spines are reduced to a row of white points along the back. The male, too, is of more robust build, its coloration is somewhat brighter, and it is more pugnacious. During the breeding season the crests in the male become at times turgid and swollen, the spines standing stiffly up, and giving the animal a much more antique and grotesque appearance. It must be noted, however, that a good deal of variation occurs in both the tints and brightness of colour in both sexes, and the spines are larger in some females than in others.

The discovery of the external characters of the sexes showed me

* 'Transactions New Zealand Institute,' vol. 10, p. 225.

that all the tuataras I had kept for the past year (twenty-nine in number) were males. This arose chiefly from the fact that males are more easily obtained than females; but it is possible that our desire to obtain the largest, most vigorous, and fully adult animals for breeding accounts in part for our having retained only males for breeding purposes.

On the occasion of this second visit I secured as many females as I could, but found more difficulty in obtaining females than males, so that I set a number of males at liberty as being superfluous. The apparently greater abundance of the males is perhaps due to the females seeking concealment more than the other sex; at any rate, I am led to suggest this from the observation of my tuataras kept in confinement. The instinct of concealment would, of course, be of special value to a female laden with eggs.

Notwithstanding that I now possessed a dozen pairs of tuataras, no eggs were obtained the following summer. The lizards had been kept in large houses and were well cared for, and appeared in good health, but would not breed. Capitivity would seem to interfere with their reproductive powers, an effect which would hardly be anticipated with animals of so sluggish a nature.

I could only refer their sterility in confinement to a change in some of the conditions of life consequent on captivity, and endeavouring, therefore, to make their surroundings approximate more closely to the natural ones, I had still larger houses constructed, and extensive runs on the open ground enclosed.

It was not, however, till January, 1889, that eggs were obtained, and even then some of them were infertile. Weary of the constant watching of the lizards in previous summers, I took a short holiday at the New Year, and during an absence of five or six days a female lizard died, but was not noticed by the attendant in charge. On my return I found that it had contained twelve fully formed eggs; they had, however, begun to putrefy. A second female laid ten eggs, which proved infertile. A third, which promised well, died from inability to lay its eggs. It was closely watched, and dissected within an hour of its death. The oviducts contained four and five eggs respectively, fully formed and ready for laying. From these eggs were obtained a number of embryos at various stages of development, from a stage equal to a two days' chick up to a stage shortly before hatching. This year I hoped to obtain a further supply of embryos, but only one female has laid, and her eggs were infertile.

The eggs of the tuatara are oval in form, both ends being of equal diameter, and vary in length from 2·5 to 3·35 cm. The egg-shell is probably much like that of other oviparous lizards, being tough, flexible, and very elastic; it contains a varying amount of carbonate of lime. The eggs dry and shrivel with great readiness when exposed to the

air, and must, therefore, be kept in damp surroundings. On the other hand, excess of moisture encourages the growth of micro-organisms in the mucus with which the eggs are frequently covered when laid, and such foreign growths tend to the destruction of the contents.

On the whole, the general features of the development are closely similar to those in other lizards; I propose, therefore, to reserve the details until a complete account can be given. I may, however, mention that the pineal eye becomes a prominent feature at an early stage. When pigment is deposited in the skin, an oval spot is left free from it over the eye, and through this the dark pigment of the retina shows clearly. Spencer* has stated that there is in *Sphenodon* very little external trace of the pineal eye. This is true of the adult, but in the recently hatched tuatara the pineal eye still shows as a dark spot through the translucent skin over the parietal foramen. This I have been able to observe even in a tuatara 8 inches in length. But as the tuatara grows older the skin over the pineal eye becomes more opaque, and though in some individuals the scantier development of the pigment over the parietal foramen affords a feeble indication of the position of the eye, yet in others the pigment is deposited there as elsewhere, so that all external trace of the eye is finally lost.

V. "On the Position of the Vocal Cords in Quiet Respiration of Man, and on the Reflex-Tonus of their Abductor Muscles." By FELIX SEMON, M.D., F.R.C.P., Assistant Physician in charge of the Throat Department of St. Thomas's Hospital, and Laryngologist to the National Hospital for Epilepsy and Paralysis, Queen Square. Communicated by Professor VICTOR HORSLEY, F.R.S. Received May 25, 1890.

(Abstract.)

The investigation which forms the subject of this paper was undertaken with a view of settling, if possible, the moot question whether in man the larynx during quiet respiration plays an active rôle or not, and, if the former, what is its function?

For this purpose the author has examined, by means of graduated mirrors, the size of the glottis during quiet respiration in fifty adult trained healthy persons, and, after death, in twenty-five adult bodies. The method of the examination and the precautions necessary to guard against possible fallacies are fully described in the paper. A comparison of the measurements thus obtained shows that in less than 20 per cent. the vocal cords during quiet respiration perform

* 'Quart. Journ. Microsc. Science,' vol. 27, p. 176.

rhythmical movements synchronous with inspiration and expiration, and that in more than 80 per cent. the glottis in both sexes forms during quiet respiration an isosceles triangle, 2—3 times as large on the average as that representing the cadaveric glottis. Under all circumstances the minimum size observed during life is larger than the maximum seen after death.

Additionally corroborative literary and experimental evidence (from experiments on animals) is adduced, showing that the glottis in man during quiet respiration is considerably wider open than after death.

The position of the vocal cords during quiet respiration, therefore, represents neither a state of inaction of their antagonistic adductor and abductor muscles nor a condition of equilibrium between them. It must necessarily be the result of actual muscular contraction, and must represent either simultaneous activity of both the adductors and abductors, with preponderance of the latter; or, secondly, some degree of activity on the part of the latter alone, the adductors being not at all in a state of functional activity.

Prior to discussing the question which of these two possibilities corresponds to the actual facts, the author investigates the *cause* of the difference found to exist between the conditions observed during life and after death. He draws attention to the fact that the larynx serves two functions, in a certain sense antagonistic to each other, viz., those of respiration and phonation. For the purposes of the former it is indispensable that the lumen of the air-tubes should be wide enough to admit of the ingress and egress of the quantity of air necessary for breathing purposes; for those of the latter, that an apparatus should be interpolated within the air-tubes which would admit of a complete juxtaposition of the voice-producing organs.

It is then shown by another series of comparative measurements that by the interpolation of the vocal apparatus (which for reasons derived from comparative anatomy must be considered to be a highly specialised addition to the respiratory system) within the air-passages their narrowest part is further reduced, when the vocal cords are at perfect rest ("cadaveric position"), to less than one-third of its natural area; and, again, by a review of experimental and pathological facts, that a reduction of the glottis to this cadaveric size involves, upon the commencement of any effort, however small, some alteration in the type of respiration.

From these facts the conclusion is drawn that the interpolation of the phonatory within the respiratory apparatus had to be counterbalanced by some arrangement which supplied the minimum of space compatible with the ingress and egress of that amount of air into the lower air-passages which is required for the purposes of what we call normal quiet respiration.

This arrangement could consist either in a rhythmical opening of the glottis synchronous with each inspiration, or in a tonic dilatation of the glottic space during both phases of respiration.

It having been shown that, though both these alternatives are actually met with, the latter is much more representative of the participation of the larynx in quiet respiration of man than rhythmic movements, it remained to be investigated, as previously mentioned, whether this tonic widening represents a state of tonic innervation of both the glottis-openers and glottis-closers, with preponderance of the former, or, on the other hand, a tonic innervation of the glottis-openers alone.

This question is discussed at length, and from anatomical, physiological, pathological, and experimental reasons the conclusion is arrived at that the glottis-openers (posterior crico-arytænoid muscles) *alone* are, during life, in a state of semi-tonic contraction.

The only remaining question, viz., whether this tonus is automatic or reflex, is answered, on the basis of experimental evidence, to the effect that in all probability the tonus of the abductor muscles is of a *reflex* character, and that the impulses acting upon their ganglionic, *i.e.*, bulbar, centres, are mainly, though not exclusively, conducted along the pneumogastric nerves. The experiments upon which this conclusion is based (division of the pneumogastric nerves below the points from which the recurrent laryngeal nerves are given off), and which were kindly performed for the author by Professor Victor Horsley, are communicated in full in the paper.

The final conclusions arrived at by the author are as follows:—

1. The glottis in man is wider open during quiet respiration (inspiration and expiration) than after death or after division of the vagi or recurrent laryngeal nerves.

2. This wider opening during life is the result of a permanent activity of the abductors of the vocal cords (posterior crico-arytænoid muscles), which therefore belong not merely to the class of accessory, but of regular respiratory, muscles.

3. The activity of these muscles is due to tonic impulses, which their centres receive from the neighbouring respiratory centre in the medulla oblongata. It is very probable that these impulses rhythmically proceed to the respiratory centre from the stimulation of certain afferent fibres contained mainly, but not exclusively, in the trunks of the pneumogastric nerves, and that they are in the respiratory centre changed into tonic impulses. The regular activity of the abductors of the vocal cords during life, therefore, belongs to the class of reflex processes. The permanent half-contraction of these muscles, in which form their tonic innervation is manifested, can be further increased, in concord with the general laws of the mechanism of respiration, by either volition or other reflex influences.

4. In spite of their extra-innervation, the abductors of the vocal cords are physiologically weaker than their antagonists.

5. These antagonists, the adductors of the vocal cords, have primarily nothing at all to do with respiration, and ordinarily serve the function of phonation only. Their respiratory functions are limited to—

(*a.*) Assistance in the protection of the lower air passages against the entry of foreign bodies.

(*b.*) Assistance in the modified and casual forms of expiration known as cough and laughing.

VI. "A Record of the Results obtained by Electrical Excitation of the so-called Motor Cortex and Internal Capsule in an Orang Outang (*Simia satyrus*)." By CHARLES E. BEEVOR, M.D., F.R.C.P., and VICTOR HORSLEY, B.S., F.R.S. (From the Laboratory of the Brown Institution.) Received June 5, 1890.

(Abstract.)

Having been engaged for some time in investigating the representation of motor function in the cortex of the bonnet monkey, we thought it advisable to perform the same in an anthropoid as likely thereby to gain a closer insight into the modes of representation in man.

We first describe the peculiarities noticeable in the configuration of the convolutions in the orang.

As in the bonnet monkey, after narcotisation with ether, we divided the cortex into squares of 2 millimetres side, and excited the same with minimal stimuli from the secondary coil of an inductorium.

General Results.—The mode of representation of motor function was found to be highly specialised. The general plan was identical with that seen in the bonnet monkey in that the representation of each segment and part of the body in the orang was arranged in the same order as that according to which we found the representation of the primary movements to be grouped in the macacque monkey.

In addition to this, the areas for the representation of the different parts of the body we found not to be continuous with each other, but that between the areas of representation (for instance, of the face and the upper limb) there were regions of inexcitable cortex showing a degree of differentiation not obtained in the lower monkey.

A further remarkable evidence of specialisation was noticeable in

the fact that excitation of any one point elicited rarely more than one movement and only of one segment, *e.g.*, simple flexion of the elbow. Consequently, any sequence of movement or march was conspicuously infrequent.

Finally, the character of each movement and its localisation was recorded.

After the cortex had been removed, we proceeded to stimulate the fibres of the internal capsule, and the results obtained confirmed those obtained from the bonnet monkey, and at the same time showed the relative position of the cortical areas.

The internal capsule was exposed by removing half of one hemisphere by a horizontal section; the outlines of the basal ganglia were then transferred to paper ruled with squares of 1 millimetre, and the resulting movement obtained by stimulating each of these squares contained in the internal capsule was recorded. The movements obtained correspond generally with the results which we have in another paper presented to the Royal Society and read on December 12, 1889.

VII. "A further Note on the Influence of Bile and its Constituents on Pancreatic Digestion." By SIDNEY MARTIN, M.D., Pathologist to the Middlesex Hospital, British Medical Association Research Scholar, and DAWSON WILLIAMS, M.D., Assistant Physician to the East London Hospital for Children, Shadwell. Communicated by E. A. SCHÄFER, F.R.S. (From the Physiological Laboratory, University College, London.) Received June 9, 1890.

Ox Bile and Pancreatic Extract.

In a previous communication* we have pointed out that in the pig the presence of bile or bile salts hastens the digestion of starch by pancreatic extract, the amount of dextrine and of sugar being considerably and proportionately increased. The same holds good for ox bile salts and extract of ox pancreas, so far at least as the increase in the amount of sugar is concerned, and for human bile and pancreatic extract (pig's). Experiments were conducted in the same manner as those with pig's secretions. In one experiment four vessels, A, B, C, D, containing 100 c.c. distilled water in which 2 grams of starch had been boiled, were taken. To B 2 per cent., and to C 4 per cent., of ox bile salts were added and dissolved. Equal quantities of glycerine extract of pancreas were added to A, B, and C,

* 'Roy. Soc. Proc.,' vol. 45, p. 358.

and the four vessels kept at a temperature of 40° C. At the end of seven minutes there was a marked difference between A and B and C in their reaction to a solution of iodine: A gave a deep blue-purple, B and C a red-purple. At the end of thirteen minutes C gave a faint red colour, B a red-purple, while A gave the same purple colour as after seven minutes. No change occurred in D. The solutions were then boiled and the amount of sugar in A, B, and C estimated as dextrose by Fehling's method:—A contained 0·526 per cent., B 0·649 per cent., and C 0·675 per cent.

The experiment was varied by using an active powdered pig's pancreatin, manufactured by Savory and Moore. Four vessels were used, each containing 50 c.c. of distilled water in which 1 gram of starch had been boiled. To B 1 per cent. and to C 2 per cent. of ox bile salts were added and dissolved. To A, B, and C, pancreatin 0·15 gram was added; D was reserved as a control. The mixture was digested for seven minutes in a water-bath at 45° C.; at the end of this time A struck a blue-purple colour with iodine, B a dirty red-purple, and C gave only a trace of red colour. The amount of dextrose was estimated by Fehling's method with the following result:—A contained 0·3846 per cent., B 0·71429 per cent., and C 0·833 per cent.

Ox bile salts and human bile have, therefore, the same property as pig's bile and bile salts; they augment the amylolytic action of the pancreatic amylopsin on starch.

Bile salts consist of a mixture, in varying proportions, of the alkaline (chiefly sodium) salts of taurocholic and glycocholic acid. In human bile, and that of most mammals, as well as in birds and amphibians, taurocholates are most abundant; in the pig, glycocholates. Experiments were therefore made separately with glycocholate and taurocholate of soda.

Taurocholate of Soda.—The salt used was a commercial product and contained some bile pigment. Four vessels, each containing 100 c.c. of distilled water in which 2 per cent. of starch had been boiled, were taken: to A 1 gram, to B 2 grams, to C 3 grams, and to D 4 grams of the taurocholate were added; 0·8 gram pancreatin was then introduced into each, and the mixture digested at a temperature of 37° for a quarter of an hour. Tested by the colour struck with solution of iodine, D had been more changed than C, C than B, and B than A. The amount of dextrose estimated by Fehling's method was A 0·809 per cent., B 1·0 per cent., C 1·05 per cent., and D 1·11 per cent. In another experiment the effect of 1 and of 3 per cent. of the taurocholate were contrasted with each other and with the effect of pancreatin alone. Digestion was continued for ten minutes at a temperature of 40—41° C., and the mixture then boiled; that containing no taurocholate contained 0·909 per cent. dextrose, that containing 1 per cent. of taurocholate contained 1·111 per cent. dextrose,

and that to which 3 per cent. of taurocholate had been added contained 1·2424 per cent. dextrose.

Glycocholic Acid.—The addition of pure glycocholic acid in the proportion of 0·5 per cent. arrested digestion of starch by pancreatin; probably because of the acidity of the mixture.

Glycocholate of Soda.—A weighed quantity of pure glycocholic acid was dissolved in distilled water and neutralised with anhydrous carbonate of sodium. Four vessels, each containing an equal quantity of distilled water in which 1 per cent. of starch had been boiled, were taken: to B 1 per cent. of glycocholic acid by weight neutralised with Na_2CO_3, and to C 2 per cent. of glycocholic acid neutralised by the same salt were added. Equal quantities of pancreatin were added to A, B, and C; D being reserved as a control. The mixtures were digested for seven minutes at 37° to 38° C. The colour struck with with iodine solution by C was then red, by B purple-red, and by A purple. Digestion was then stopped by boiling and the quantity of dextrose estimated by Fehling's method: A contained 0·357 per cent., B 0·476 per cent., C 0·588 per cent.

Glycocoll, Leucin, and Tyrosin.—Glycocholic acid is formed by the conjunction of glycocoll and cholalic acid, glycocoll itself being amido-acetic acid. Leucin and tyrosin, the end-products of pancreatic digestion, are also amido-acids, leucin being amido-caproic acid, and tyrosin, oxyphenyl-amido-propionic acid. Glycocoll was found to be without any effect upon the pancreatic digestion of starch.

Leucin appeared to interfere to some extent with pancreatic digestion of starch; thus, in one experiment, in which 0·5 per cent. of pure leucin was added to a starch mixture and digested with pancreatin for twelve minutes, the amount of sugar estimated as dextrose was 0·526 per cent., while the amount in a similar mixture digested for the same time without leucin was 0·645 per cent.

Tyrosin also appeared to interfere slightly with pancreatic digestion of starch. Thus, in one experiment three vessels were taken, each containing 100 c.c. of distilled water in which 1 gram of starch had been boiled; to flask B 0·05 gram of pure tyrosin and to flask C 0·1 gram tyrosin were added; the three mixtures—A, to which no tyrosin was added, B, and C—were then digested for nine minutes with equal quantities of pancreatin. At the end of five minutes the colour struck with iodine solution varied, A giving a reddish-purple, C and D a bluish-purple; at the end of eight minutes the colour with A was almost pure red, with B and C still a bluish-purple. The quantity of sugar estimated as dextrose by Fehling's method was as follows:—A 0·383 per cent., B 0·345 per cent., and C 0·333 per cent.

Carbonate of Sodium.—Carbonate of sodium, when present in the proportion of 0·25 per cent. and over, retards pancreatic digestion of

starch. By experiments conducted as those above detailed, we have found that this retardation occurs also in the presence of bile salts, although it is not so great as with the carbonate alone. In one experiment four vessels were taken, each containing distilled water in which 2 per cent. starch had been boiled, and 1 per cent. bile salts subsequently added and dissolved, to B 0·25 per cent. Na_2CO_3, to C 0·5 per cent. Na_2CO_3, and to D 1 per cent. Na_2CO_3. The mixtures were digested with equal quantities of pancreatin for nine minutes at 39° C., boiled, and neutralised. The amount of dextrose as estimated by Fehling's method, was :—In A 0·83 per cent.; in B 0·55 per cent.; in C 0·492 per cent.; in D 0·3773 per cent. Even in the presence of an excess of carbonate of sodium, however, the addition of bile salts does favour the progress of pancreatic digestion of starch, as shown by the following experiment. Four vessels, each containing equal quantities of distilled water in which 2 per cent. of starch had been boiled, were taken; to B and D 1 per cent. bile salts were added and dissolved, to C and D 0·5 per cent. carbonate of sodium was added and dissolved. The mixtures were digested with equal quantities of pancreatin for 11 minutes at a temperature of 37° C., and then boiled and neutralised. The amount of sugar estimated as dextrose by Fehling's method was :—

A. Pancreatin	....	0·695 per cent.
B. Pancreatin + bile salts	....	0·952 ,,
C. Pancreatin + Na_2CO_3	....	0·208 ,,
D. Pancreatin + bile salts + Na_2CO_3	....	0·384 ,,

Digestion of Proteids in the Presence of Bile.

Experiments were also made to test the influence of bile on pancreatic proteolytic digestion.

Bile Salts.—The fluid to be digested was made by diluting egg-albumen with distilled water, agitating, neutralising with acetic acid, and straining the resultant mixture through muslin. Measured quantities of this albuminous fluid were at the time of experiment coagulated by heat and one or two drops of acetic acid; the digestion was conducted in the same vessel as coagulation was effected.

Experiment I.—Three beakers, A, B, and C, each containing 120 c.c. of diluted egg-albumen, coagulated in the manner above described, were taken, and to each was added 1 per cent. of sodic carbonate; to A 2 per cent. of bile salts of the pig was added and dissolved; to A and B 1 gram of pig's pancreatic extract rich in proteolytic ferment was added, and all three beakers placed in a warm chamber at 35° C., and digested for three hours. The albumen in A at the end of that time appeared to be much more digested than that in B; that in C was unchanged. The fluids were then rapidly boiled, to stop all

ferment action. A contained a deep yellow-coloured turbid fluid, with a slight flocculent white precipitate; B a light yellow-coloured turbid fluid with copious white precipitate. The three mixtures were then filtered through double, balanced filters, and the filter washed, first with boiling distilled water, then with boiling methylated spirit, and finally with absolute alcohol. The filters were then dried at 120° C, and weighed.

A, the fluid which contained bile salts, yielded a residue weighing 0·150 gram.

B, the fluid which did not contain bile salts, yielded a residue weighing 0·536 gram.

C, which was not digested, gave a residue weighing 1·256 grams.

Experiment II.—In this a larger proportion of bile salts was used, and digestion was conducted at a higher temperature. Equal quantities of egg-albumen diluted with distilled water were introduced into three beakers, A, B, and C, and coagulated by heat and a few drops of acetic acid; 1 per cent. of sodium carbonate was added to each, and to A 3 per cent. of pig's bile salts; to A and B 1 per cent. of pancreatic extract rich in proteolytic ferment was added, and the mixture digested at about 40° C. for three hours. The fluids were then boiled, and subsequently filtered, the precipitates being washed and dried as in Experiment I. The weights were as follows :—

A, the fluid which contained bile salts, yielded a residue weighing 0·098 gram.

B, the fluid which did not contain bile salts, yielded a residue weighing 0·665 gram.

C, the fluid which was not digested, yielded a residue weighing 1·062 grams.

Glycocholate of Soda.—The effect of glycoholate of soda appeared to be less marked than that of the bile salts as a whole. Thus, in experiments conducted in the same manner as those above described, a small portion of glycocholate of soda appeared to have the effect of slightly increasing the amount of albumen dissolved, while a somewhat larger proportion either had a slightly contrary effect or none at all. In one experiment equal quantities of albumen were taken, and glycocholic acid,* 0·5 per cent., added to one vessel (C), and 1·0 per cent. to another vessel (D), while none was added to a third vessel (B), and the three fluids were then digested with pancreatin for two hours. A fourth vessel was retained as control. The fluids were filtered through balanced filter papers, and the filter washed with distilled water, boiling methylated spirit, and absolute alcohol, and dried at 110° C.

The weights of the residues were as follows :—

* The acid was neutralised with Na_2CO_3.

A ...... 0·625 gram.	B ...... 0·539 gram.
C ...... 0·514 „	D ...... 0·541 „

These experiments show that the power of bile to hasten pancreatic digestion is not limited to amylolytic digestion, but that it is equally, if not more, marked in its influence on proteolytic digestion.

VIII. "On the Spectra of Comet *a* 1890 and the Nebula G.C. 4058." By J. NORMAN LOCKYER, F.R.S. Received June 12, 1890.

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"On the Chief Line in the Spectrum of the Nebulæ." By J. NORMAN LOCKYER, F.R.S. Received December 9, 1889, —Read January 16, 1890. Revised May, 1890.

I. INTRODUCTORY.

In 1887, reasoning from the spectral phenomena in stars of Vogel's Class III*a*, which my researches proved to be due to the mixture of bright flutings of carbon and dark flutings of manganese and other substances, I came to the conclusion that these stars could not be stars in the ordinary sense but swarms of bodies separated from each other.

I next showed, reasoning again from the spectral phenomena, that these bodies were in all probability meteorites or particles of meteoritic dust.

A discussion of the origin of such stars as these next suggested that it must be sought for in the nebulæ. Meteoritic dust was then experimented upon and two lines of unknown origin in the spectrum of this dust were found to be roughly in the same position as two lines of unknown origin in the nebula spectrum.

In my first communication to the Royal Society on this subject I stated that the conclusions were "given with great reserve," and I was careful to point out that I had limited myself to small dispersion (1 prism of 60°), because it was imperative that all the observations should be strictly comparable; those of very faint glows visible with difficulty and those of a bright electric arc, to speak only of laboratory work; and I also added that there was an additional reason for this in the difficulty of obtaining astronomical observations with large dispersions in the case of very dim celestial objects.

Seeing, therefore, that I dealt only, of set purpose, with small dispersion, I limited myself to a "short title" of three figures in my references to the lines.

But, although I did not employ great dispersion in the first instance, I fully understood that this must be done eventually, so I at

once provided for such observations both in laboratory and observatory, and they were commenced last May.

So far, however, only one branch of the observatory work has been commenced, in consequence of delays on the part of the instrument maker. I should here state that my main endeavour in one direction will be to obtain photographs of the spectra of nebulæ and reference spectra under such conditions that any instrumental error will register itself on the plate in such a way that a proper correction for it can be made. For such work as this much light and great stability are required; I have therefore erected a 30-inch reflector at Westgate-on-sea, having received a grant in aid from the Government Grant Fund; the mirror has been figured and presented to me by my friend Mr. Common; the flat (7 inch diameter) by other friends, the Brothers Henry, and I am anxious to take this opportunity of expressing my obligations to them for this magnificent help in my work.

The laboratory researches, consisting of the application of higher dispersion, have made more progress; but they are not yet finished, and in this paper I shall confine myself to observations made with reference to the chief nebula line.

A paper by Dr. and Mrs. Huggins, which has appeared in the 'Proceedings' during the recess, contains criticisms of some points in my recent papers which require a reply. They hold that I am wrong in my identification of the origin of some lines in the spectrum of the nebulæ, chiefly those two which Dr. Huggins himself has formerly ascribed in one case to an unknown form of nitrogen and in the other to hydrogen under some conditions which we cannot match in the laboratory; I, on the other hand, suggesting that possibly they may be produced by magnesium, a substance which occurs in most meteorites.

I gather from this paper of Dr. and Mrs. Huggins that my object in using a three-figure reference to the lines has been misunderstood. I think it important, therefore, that I should at the present time return to the subject, giving my reasons for the three-figure references I used in the first instance. For this purpose it is necessary to state the history of the subject less eclectically than Dr. and Mrs. Huggins have done. I shall then give the work that has been since accomplished, though, as I have stated above, it is not yet finished; and reply to Dr. and Mrs. Huggins's criticisms as best I can.

As in my replies to the various objections raised by Dr. and Mrs. Huggins, I am most anxious not to even unconsciously misrepresent their views, I shall deal with each line separately, take each objection *seriatim*, and give their own words as far as possible. I confine myself in the present paper to that at λ 500.

I am extremely gratified to find that in an inquiry dealing with, roughly, some 10,000 observations, which has taken me two years, and the details of which occupy some 235 pages in the 'Proceedings,' the point in my argument to which Dr. and Mrs. Huggins take exception is a subsidiary one.

II. The Accuracy Attained in Earlier Investigations.

My astronomical work has so far been almost exclusively devoted to the Sun, in which case considerable dispersion is easily utilised. When my Sun work drove me to try to obtain some information from other celestial bodies, I had to enter a comparatively unfamiliar field of observation. It may well be, therefore, that I ignorantly over-estimated the difficulties of such observations, and a passage in Dr. and Mrs. Huggins's paper seems to suggest that such is the case. They refer to observations of nebulæ with a dispersion approaching that given by 8 prisms of 60°, with the Royal Society telescope. It must be pointed out, however, that although this instrument has been in Dr. Huggins's possession for nearly twenty years, so far as I know no such observations have been continuously made with it previously, or indeed by any other instruments in the hands of any other observers. I was justified in this view by noting that in Dr. Huggins's important research published in 1879 only one prism was employed in obtaining the photographic spectra of some of the brightest stars in the heavens, made with the same telescope, and that some two years afterwards, in 1881, he wrote, with reference to some observations made by him of the comet of that year:—"I am also able to see upon the continuous solar spectrum a distinct impression of the group of lines between G and *h* which is usually associated with the group described above. My measures for the less refrangible group give a wave-length of 4230, which agrees, as well as can be expected, with Professor Liveing and Dewar's measures, 4220."* A diagram of the spectrum of the comet was published in the paper in which this passage occurs, to which some importance seems to have been attached.

As, judging from this, the position of the lines in question could have been read to about two in the fourth place, I am justified in regarding this statement as a practical use of a three-figure reference. I gather from Dr. and Mrs. Huggins's criticism that Dr. Huggins now expresses wave-lengths by five figures, that is, he states wave-lengths to the hundred-millionth of a millimetre It is convenient, therefore, to express the discrepancies between different measurements in terms of this quantity as a unit. It will be seen that in 1881 he accepted with complacency a variation of 100 units of such

* 'Roy. Soc. Proc.,' vol. 33, p. 2.

a scale in the measurements made in the laboratory and observatory respectively of a line which his diagram shows as clearly defined, though in the text he uses the ambiguous phrase that he was able to see a "distinct impression" of it. He did not consider that a discrepancy of the magnitude indicated threw any doubt upon the identity of the lines.

I may mention as another fact which supports the use of a three-figure reference, that even in the research on the brightest stars it was difficult to absolutely reconcile the one prism work in the observatory with laboratory work. No better illustration of this could be found than a comparison of the work of two such observers as Dr. Huggins and Prof. Cornu.

One of the chief points in the memoir was the discovery of a series of lines in the ultra-violet which Dr. Huggins ascribed to hydrogen; lines near the position given have, indeed, since been measured in hydrogen by Cornu.* I append the wave-lengths as given by the two observers:—

Huggins.†		Cornu.
3767·5	....	3769·4
3745·5	....	3749·8
3730·0	....	3733·6
3717·5	....	3720·6
3707·5	....	3710·7
3699·0		

It will again be observed, that if one had wished to give a handy reference to these lines—a short title—three figures would have been sufficient in two cases, for we have—

3717·5	....	3720·6
3707·5	....	3710·7

It is no part of my present business, however, to discuss the relative accuracy of Dr. Huggins and Prof. Cornu as observers, but it must be pointed out that by the nature of the research Prof. Cornu is more likely to be right. We owe one of our best maps of the solar spectrum in this part to Prof. Cornu, and the comparison of hydrogen with the Sun could always be repeated at leisure and under stable conditions, whereas, in the case of Dr. Huggins, the result depended upon a photographic solar comparison in the telescope taken some hours afterwards. In any case, Professor Cornu's numbers are three years old, and, so far as I know, Dr. Huggins has not challenged them.

An inspection of the numbers shows that there is, in all probability,

* 'Journal de Physique,' vol. 10, 1886, 341.

† 'Phil. Trans.,' vol. 171, p. 682.

a systematic error in Dr. Huggins's results of about thirty of the units suggested above, and anyone acquainted with spectroscopic work will see how very easily this might arise from the absence of perfect adjustment.

This brings me to another point which also influenced me in arriving at the view I held, however erroneously.

For the last fifteen years I have been employed, among other matters, in taking photographs of the solar spectrum compared with arc spectra. Of the thousands of photographs taken (with a dispersion such that the distance between H and K covers about half an inch on the plate) many hundreds have been rejected on account of the want of exact coincidence between the solar and terrestrial lines of the same element, the slightest variation in the rate of the clock of the heliostat or siderostat employed, or the occasional changing of the arc from the centre to either side of the pole, being enough to produce this result.

Hence, when I wrote my paper of November, 1887, I held (and I still hold, although I may have erred in overrating the difficulty of observing stellar and nebular spectra) that short titles of the lines compared, extending to three figures, sufficiently refer to positions, and do not really underestimate the accuracy generally attainable. Indeed, if this be not so, then from the single instance I have quoted Dr. Huggins's classic paper on the spectra of the white stars is misleading, and his series of lines in the ultra-violet cannot be due to hydrogen.

These general remarks being premised, I next give in historical sequence the observations of the nebula line now in question.

III. Wave-length of the Chief Nebula Line.

A. *Historical Notice.*

Dr. Huggins's considers the nebulæ to be masses of gas, and he has suggested that the chief nebula line may owe its origin to some unknown form of nitrogen.

When I commenced my experiments on meteoritic glows, I saw a line in the position of the chief nebula line, with the dispersion employed. Thinking it might arise from the magnesium in the silicate, I tried terrestrial olivine, and I again saw the line in its spectrum. Subsequent work with a large model Steinheil spectroscope (four prisms and a high power eye-piece) showed that the line was coincident with the least refrangible member of one of the flutings seen in the flame-spectrum of magnesium, the wave-length of which had been given as follows :—

*Lecoq de Boisbaudran	5006
†Watts	5006·5
‡Liveing and Dewar (1878)	5000
§ „ „ (1888)	5006·4

Now for the determination of the wave-length of the chief nebula line. I give a condensed statement of the observations available when my paper was written.

(1864) :—"The strongest line coincides in position with the brightest of the air lines."|| The diagram which accompanies this description represents the line about midway between the two components of the bright, coarse, nitrogen double. The wave-lengths given by Dr. Watts for the nitrogen lines from a reduction of Dr. Huggins's measures are 4999 and 5003;¶ so that, according to these measures, the wave-length of the nebula line would be somewhere between those positions.** Taking Thalén's measures of the nitrogen lines (5002 and 5005), the position of the nebula line —assuming that it fell according to the drawing—would be 5003·5.

Taking Thalén's measures and Dr. Huggins's reference to the coarse double line, which with the dispersion then used appeared as a single one, the wave-length might be 5002·1 or 5005·1 or any value between these. We have thus a limit of error of thirty units by Thalén's values. Kirchhoff's values for the two lines, as given by Watts, are 5004·6, 5000·6.

1865. Secchi observed the spectrum of the Orion nebula in 1865.††

* 'Spectres Lumineux,' p. 86.

† 'Phil. Mag.,' 1875, p. 85.

‡ 'Roy. Soc. Proc.,' vol. 27, p. 353.

§ 'Roy. Soc. Proc.,' vol. 44, p. 242.

|| Dr. Huggins, 'Phil. Trans.,' 1864, p. 438.

¶ 'Index of Spectra,' p. 3.

** Dr. and Mrs. Huggins, in their paper, call these figures of Watts into question. They say, "Watts's reduction of my (*sic*) measures to wave-lengths is clearly not accordant with the measures of air-lines immediately preceding and following this line. I have therefore reduced my original measures to wave-lengths, and find for N_1 the value 5004·5—

"Kirchhoff............	5004·6
"Thalén	5005·1

"Thalén's value is clearly too high, as Thalén gives for the lead line coincident with N_1 λ5004·6 and N_1 is seen on the more refrangible side of the solar iron line given by Ångström as λ5004·9. In Ångström's map N_1 is laid down on the more refrangible side of the iron line 5004·9 at about 5004·5. The same position is given to N_1 in Kirchhoff's map. I have made a new determination of the position of N_1, using the second spectrum of a grating 17,300 to the inch, relatively to the solar iron line at 5004·9 according to Ångström. The value came out 5004·6." ('Roy. Soc. Proc.' vol. 46, p. 45.)

†† 'Comptes Rendus,' vol. 60, p. 543.

Three lines were seen, and others suspected. The positions of the lines were only roughly determined even by such an experienced observer. The strongest and widest of the lines was described as situated at two-thirds of the interval between F and *b*, whilst the second line appeared coincident with F.

1868. An observation made by Dr. Huggins in 1868 reads:—"The determination of the position in the spectrum of the three bright lines was obtained by simultaneous comparison with the lines of hydrogen, nitrogen, and barium. The instrument which I employed had two prisms, each with a refracting angle of 60°, *and the positions of the lines were trustworthy within the limits of about the breadth of the double line* D . . . . The coincidence of the line in the nebula with the brightest of the lines of nitrogen, though now subjected to a much more severe trial, appeared as perfect as it did in my former observations.*

It will be noticed that in these observations Dr. Huggins informs us to what extent his observations were trustworthy, and, taking Thalén's measures for D, viz.,

5895·0
5889·0

we find the possible error to be sixty units of the scale above suggested. In the diagram which accompanies the above description the nebula line is shown coincident with the *less* refrangible component of the nitrogen double, in contradistinction to the former observation, which, made with less powerful dispersion and in accordance with Dr. Huggins's estimate of the accuracy attained at that time, placed the line midway between them.

In another paragraph of the same paper (p. 543) Dr. Huggins takes "the wave-length of the nitrogen line at 500·80 millionths of a millimetre." Hence, according to this statement, the nebula line would have a wave-length of 500·80; or 500·51, if Thalén's value for the less refrangible nitrogen line be taken, and by Dr. Huggins's own assertion this value would only be accurate within the interval between the sodium double D, that is, 0006·0. It should also be noticed that the double line of nitrogen is again referred to as if it were a single line.

1868. Lieutenant Herschel made some micrometric measures of the chief nebula line in 1868,† and a reduction of them was made by D'Arrest in 1872, with the following result:—‡

* 'Phil. Trans.,' 1868, pp. 541–2.

† 'Roy. Soc. Proc.,' vol. 16, p. 451.

‡ 'Undersögelser over de Nebuloseestjerner,' Copenhagen, 1872, p. 22.

Gen. Cat. No.	λ of chief line.
1179 Orion neb.	501·7
1567	501·0
2102	500·8
2197	493·4
2581	499·8
2917	500·6
4066	499·8
4361	497·2
4390	504·9
4403	499·8
4407	499·4
4510	504·4
4628	501·9

The mean wave-length given by this series is 500·36, and the extreme values are 493·4—504·9.

1871. The following observations of the line in question were made by Vogel in 1871.*

Orion nebula, 13th January, 1871	500·3
„ „ 19th March, 1871......	500·2
General Catalogue 4234	500·5
„ „ 4373	500·7
„ „ 4390	500·5
„ „ 4447	500·7
„ „ 4510	500·8

These measurements differ by no less than 60 units of the scale now adopted; the mean value is 500·53.

1872. Dr. Huggins writes: "The line of nitrogen, when compared with it (the nebula line), appeared double, and each component nebulous, and broader than the line of the nebula. This latter line was seen on several nights to be apparently coincident with the middle of the less refrangible line of the double line of nitrogen."†

This observation, however, obviously left us in the same position as that of 1868, as far as the wave-length of the nebula line was concerned.

1874. In a paper "On the Motions of some of the Nebulæ towards or from the Earth" communicated to the Society in 1874, Dr. Huggins wrote:—‡

"The brightest line in the nebular spectrum is not sufficiently coincident in character and position with the brightest line in the

* 'Bothk. Beob..' Leipzig. 1872.

† 'Roy. Soc. Proc.,' vol. 20, p. 383.

‡ 'Roy. Soc. Proc.,' vol. 22, p. 252.

spectrum of nitrogen to permit this line to be used as a fiducial line of comparison. The line in the spectrum of the nebulæ is narrow and defined, while the line of nitrogen is double, and each component is nebulous and broader than the line of the nebulæ. The nebular line is apparently coincident with the middle of the less refrangible line of the double line of nitrogen* . . . In the course of some other experiments, my attention was directed to a line in the spectrum of lead which falls upon the less refrangible of the components of the double line of nitrogen. The line appears to meet the requirements of the case, as it is narrow, of a width corresponding to the slit, defined at both edges, and in the position in the spectrum of the brightest of the lines of the nebulæ.

"In December, 1872, I compared this line directly with the first line in the spectrum of the Great Nebula in Orion. I was delighted to find the line sufficiently coincident in position to serve as a fiducial line of comparison.

"I am not prepared to say that the coincidence is perfect; on the contrary, I believe that, if greater prism power could be brought to bear upon the nebulæ, the line in the lead spectrum would be found to be in a small degree more refrangible than the line in the nebulæ.

"The spectroscope employed in these observations contains two compound prisms, each giving a dispersion of 9° 6′ from A to H. A magnifying power of sixteen diameters was used.

"In the simultaneous observation of the two lines it was found that, if the lead line was made rather less bright than the nebular line, the small excess of apparent breadth of this latter line, from its greater brightness, appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible sides of the two lines appeared to be in a straight line across the spectrum. This line could be therefore conveniently employed as a fiducial line in the observations I had in view."

1877. The measures, by Dr. Copeland and Lord Lindsay, of the wave-length of the line near 500 in Nova Cygni, which has generally been accepted as the nebula line, were as follow :—†

1877, January		2	......	502·4
,,	,,		......	505·1
,,	,,	8	......	502·9
,,	,,	9	......	500·7
,,	,,	27	......	500·8
			Mean	502·4

* 'Roy. Soc. Proc.,' vol. 20, p. 380.
† 'Copernicus,' vol 2, p. 101.

Four additional measurements were made by Dr. Copeland and Lord Lindsay respectively on September 2, 1877. The reduction of the micrometric measures by means of curves gave the following wave-lengths :—

	Lord Lindsay.		Dr. Copeland.
	499·5	....	498·6
	500·1	....	496·2
	498·5	....	496·4
	499·0	....	497·2
Mean	499·3	....	497·1

Here, again, it is obvious that the wave-length of the line was by no means certain *even to the first three figures.*

1880. Dr. Copeland observed the spectrum of a new planetary nebula in 1880, and obtained the following measures for the chief nebula line :—*

1880, December 3......	501·1
„ 6......	501·2

Dr. Lohse measured the line at 500·6.

The spectrum of the Stephen-Webb nebula was also observed at the same time, and the line measured at 501·9.

From these observations the mean wave-length of the nebula line is found to be 501·2, a value differing widely from that given by Dr. Huggins.

1882. In Dr. Huggins's important paper on the photographic spectrum of the Orion nebula the only reference to the chief line is as follows :†—"The brightest line, wave-length 5005, is coincident with the less refrangible component of the double line which is strongest in the spectrum of nitrogen." The change of wave-length from 500·80, the value given in 1868, due to a change in the assumed value of the nitrogen line is made without explanation, which shows that Dr. Huggins did not at that time attach as much importance to such variations as he now seems inclined to do. The latest measures of this line, so far as I know, are those given by Dr. Copeland in 1888.‡ Although the dispersion employed is not definitely stated, it is remarked that "a sufficiently powerful spectroscope was used." The measures he gives are as follow :—

1886, December..	5007	Two measures
1887, January....	5003	„ „
1887 „	5003	One measure.

* 'Copernicus,' vol. 1, p. 2.

† 'Roy. Soc. Proc.,' vol. 33, p. 427.

‡ 'Monthly Notices,' vol. 48, p. 360.

In these observations, therefore, by one of our most skilled spectroscopists, we have a difference of 40 of the units now adopted, and I cannot refrain from pointing out that either the difficulties of the observations or the liability to instrumental error must be very considerable when we see such variations as these, "a sufficiently powerful spectroscope" and the magnificent instrument of Lord Crawford's observatory being employed.

It will be seen from this short retrospect—

(1.) That the mean of the recorded observations of the magnesium fluting placed it at 5004·7, while Dr. Huggins's last description (that I had seen) of the position of the nebula line in terms of wave-length gave 5008·0, as he stated it, or 5005·1, as it may be stated if we take Thalén's value for the nitrogen line. These observations, according to his own statement, were only trustworthy within a limit of sixty units, while the distances from the magnesium fluting are thirty-three and four units respectively, according to which measure of the nitrogen line be taken. From the facts at my disposal, it was obvious that, if any difference existed, the magnesium fluting was more refrangible than the nitrogen line, and therefore than the nebula line, assuming the accuracy of Dr. Huggins's observation of 1868.

(2.) That, if observations by others be considered, the wave-length of the magnesium fluting lies well within the extreme limits; and, indeed, not far from the mean of them all.

From these facts, I trust it will be seen that I was perfectly justified in stating the wave-length of the chief nebula line to three figures only, and, further, that the coincidence between it and the magnesium fluting was sufficiently probable to justify the making of a statement "with reserve" to that effect.

Since my paper of 1887, however, was presented to the Royal Society, I gather from Dr. Huggins's criticisms that he has entirely changed his ideas of the accuracy possible in these inquiries, and now practically withdraws all the statements on which I depended to form an estimate of the amount of accuracy that could be counted upon, and the instrumental means that could be employed, in these researches. As I have shown, the accuracy which Dr. Huggins had attained with all his known skill in his last published observation of the position of the nebula line was trustworthy only within sixty units, according to his own statement; this was in 1868. During the last twenty years, so far as I can make out, this observation has not been improved upon by the more powerful aids to investigation now in his possession, while, on the other hand, as recently as 1881, he regarded with complacency, as I have before stated, a variation of 100 units between the measured place of the same line in laboratory and observatory. Further, in all his impor-

tant work since 1864 Dr. Huggins has employed only one or two prisms as a rule, whilst now he states that he can use a dispersion equal to nearly eight prisms of 60° in the case of the nebula in Orion, and its use implies that this is the minimum dispersion that should be used. I am rejoiced that this is so, if it be so; and future observers, travelling over the ground of which I have attempted to make a rough survey, will no doubt have better observations to work upon than those on which I have depended. But although I am rejoiced that increased dispersion is possible, I am so thoroughly acquainted now with instrumental pitfalls that I cannot accept Dr. and Mrs. Huggins's new value until we know more exactly how it has been obtained, and until many observations, the conditions of which are more fully described, have confirmed it.

Dr. and Mrs. Huggins do not appear to have applied the same test at the same time to the coincidence of the third nebula line with the F line of hydrogen, so that whether the non-coincidence of the magnesium was due to an instrumental error cannot be determined with the facts before us.

The observed difference between the nebula line and the magnesium fluting was nineteen of Dr. Huggins's present units, so that, after all, if we only take his recent observations into account, we have better evidence for the existence of magnesium in the nebulæ than we have for hydrogen in the white stars, so far as is evidenced by the lines discovered by Dr. Huggins (see *ante*), for in their case the coincidences do not occur within thirty units.

I next refer to my own observations with high dispersion.

B. *Laboratory Observations with High Dispersion in connexion with the Chief Nebula Line.*

Dr. Huggins's observations having led him to the conclusion that the chief nebula line is coincident with the less refrangible member of the double line of nitrogen near 500, and not with the magnesium fluting, I first directed my attention to observations of these lines and flutings in the laboratory, as the arrangements for observatory work with high dispersion were not completed.

The laboratory work was begun last May, and some of the photographic results were exhibited at the Royal Society Soirée in the same month. It was, however, interrupted till the end of July, but since the recess it has been taken in hand again. Dispersions varying from that given by a Liveing direct-vision spectroscope to that of a Rowland grating of 12 feet 10 inches radius and 9·6 square inches surface, with an eyepiece of 1·4 inches equivalent focus, have been employed, in addition to which a Steinheil spectroscope with three or

(in some observations) four prisms and a Cooke spectroscope of sixteen prisms have been used.

The comparisons so far employed by Dr. Huggins in his observations of the chief nebula line are the double green line of nitrogen, a line of lead, and the bright fluting seen in the spectrum of burning magnesium. The relative positions of these have been re-observed in the laboratory.

The exact wave-length of the brightest edge of the magnesium fluting was first determined by means of a comparison photograph of the Sun and burning magnesium.

The dispersion and width of slit were such that practically all the lines seen in Rowland's photographic map were shown in the photograph. There was a slight shift, the amount of which could be determined by measuring the displacement of *b*; when this was allowed for, the wave-length of the magnesium fluting was found to be 5006·5 on Ångström's scale. This has since been confirmed by observations with the four-prism Steinheil and the first order spectrum of the Rowland grating.

Comparison photographs have also been attempted with the Rowland grating, but it was found that even with two hours' exposure only the first four maxima of the magnesium fluting were obtained. It was found difficult to keep the flame of the burning wire sufficiently steady to ensure the light falling directly on the slit during the whole time of exposure.

It may be mentioned here that the secondary maxima of the fluting succeed each other at gradually increasing distances apart. The wave-lengths given by Messrs. Liveing and Dewar* and those determined from the photographic comparison referred to are as follow :—

Liveing and Dewar.		Lockyer.	
5006·4		5006·5	
	10·8		10·4
4995·6		4996·1	
	10·2		11·0
4985·4		4985·1	
	11·8		11·6
4973·6		4973·5	
	12·0		12·2
4961·6		4961·3	
	13·0		12·9
4948·6		4948·4	
	14·2		14·0
4934·4		4934·4	

The next observations were made with respect to the relative positions of the magnesium fluting and the less refrangible component of

* 'Roy. Soc. Proc.,' vol. 44, p. 248.

the nitrogen double, which according to Dr. Huggins is coincident with the nebula line. As in his observations of the nebula Dr. Huggins used eight prisms of 60°, the Cooke spectroscope with eight prisms and a telescope magnifying fifteen times was first employed. An electric spark between magnesium electrodes was used, and the length of spark was so adjusted that the nitrogen lines were visible when a Leyden jar was connected with the coil and the magnesium fluting when the jar was taken out of the circuit. The spark was placed about 30 inches in front of the slit, and an image formed by a lens of about 9 inches focus. In this way the chances of error in measurement, due to changes in the direction of the light-source, with respect to the slit, were reduced to a minimum. The spectrum was faint, so that it was found necessary to have the slit rather wide. Under these conditions the magnesium fluting fell on the less refrangible member of the double green air line, as Dr. Huggins observed the nebula line to do in 1868; this was confirmed by my assistants, and was seen by my colleagues Professors Thorpe and Rücker. Twelve and sixteen prisms were subsequently used, and with the wide slit, which it was then necessary to employ, the magnesium fluting still fell on the less refrangible line of nitrogen.

I would here suggest that in future comparisons of the spectra of the nebulæ with that of magnesium the quantity spark should be employed for obtaining the fluting, as it is in no way fatiguing to the eye.

This comparison was repeated with a Steinheil spectroscope with three prisms of 45° and a telescope magnifying sixteen times. In this case there was less light lost than with the Cooke spectroscope, and the observations were made with less difficulty.

A small quantity of lead chloride was also introduced into the spark, and the lead line was seen to be slightly more refrangible than the edge of the magnesium fluting, so as to form a close double with it. Using a small jar, it was found possible to obtain together the spectrum of nitrogen, magnesium, and lead superposed, and under these conditions the magnesium fluting was seen still apparently coincident with the less refrangible nitrogen line, and the lead line was a little more refrangible.

The air spark so far employed was that obtained by using a small jar; the nitrogen lines were very fluffy and the spark was so feeble that it was always necessary to use a wide slit.

In subsequent experiments the jar spark between the two platinum poles inserted in a glass tube containing air at a slightly reduced pressure was used. This gave the nitrogen lines very much thinner than the ordinary spark in air, and when a larger jar was put in circuit the spectrum was also brighter. A narrower slit could therefore be used and comparisons made with greater accuracy.

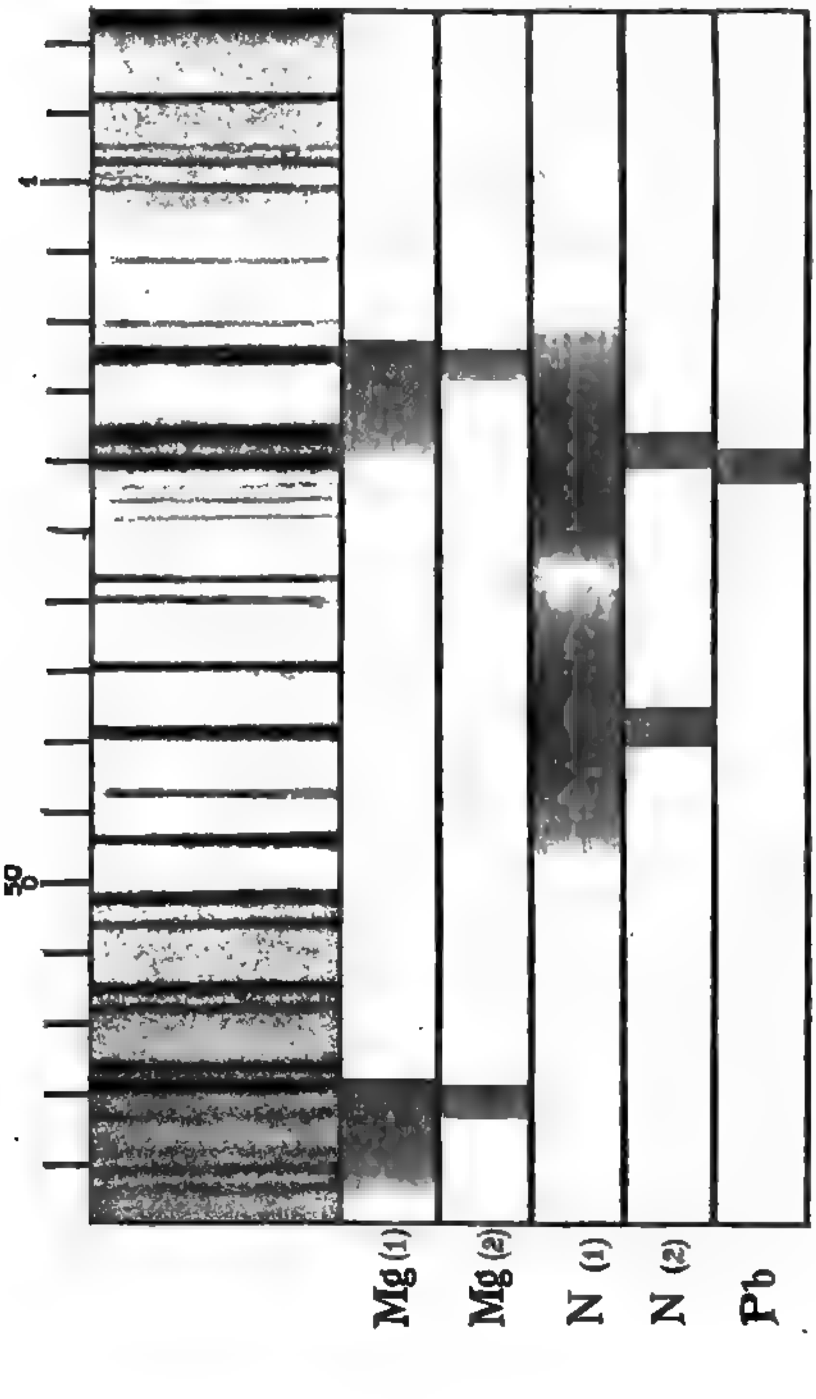


Fig. 1.—Comparison of the spectra of magnesium, nitrogen, lead, and Sun. Mg (1) and N (1) are as observed with a wide slit. (The scale is that of Rowland's photographic map.)

Four prisms and an observing telescope magnifying about thirty times were first used with the new conditions, and it was then found that the magnesium fluting was a little less refrangible than the nitrogen line. In this case the magnesium spectrum was obtained by burning magnesium in front of the bulb, and the non-coincidence with the nitrogen line was put beyond doubt by observing the two spectra simultaneously. This was further confirmed with the Rowland grating, the magnifying power employed being about fifty-five times.

Comparisons with the solar spectrum were then made, with the result shown in fig. 1.

The solar spectrum shown in the drawing is from an enlargement of Rowland's map of the region in question, and the positions of the fluting of magnesium and the lines of nitrogen and lead are as determined with four prisms and confirmed with the Rowland, which differs from Ångström's scale by one division; *e.g.*, 5007·5 on Rowland's scale is equivalent to 5006·5 on Ångström's. (This difference was determined by a comparison of twelve lines of iron mapped by Thalén with the corresponding solar lines shown in Rowland's map.) Two spectra of nitrogen are shown in the map, the first one, that seen when the small jar was used and the slit was rather wide; and the second, that seen with the jar spark in rarefied air and the slit as narrow as possible. In the first case the lines are very broad and begin to fade away rather suddenly on both sides. Two spectra of magnesium are also shown, one with the slit wide and the other with it narrow.

It will be seen that when the slit is rather wide the haziness of the less refrangible nitrogen lines overlaps the first maximum of the magnesium fluting, when seen with the same slit. This is the same whether magnesium or platinum poles were used for the air spark, and this shows that the apparent coincidence is not due to the remnant of the magnesium fluting being superposed on the air lines. The importance of using a narrow slit and a spark with large jar, preferably in air at a reduced pressure, for comparison with the nebulæ is, therefore, obvious.

The results given are not to be absolutely relied upon, as there may be slight errors, due to the various light sources not being perfectly in the line of collimation. It has been found, for example, that a change of 111 minutes of arc in the direction of the beam from the siderostat displaces the lines about two divisions of Rowland's scale, or more than the difference between the positions of the chief nebula line and the fluting of magnesium as determined by Dr. Huggins. Every precaution was taken, however, to ensure the accuracy of the observations. The beam from the siderostat was first directed on the slit, and the spark and lens placed in the same direction by adjusting

their shadows on the slit plate. The slit was made as narrow as the luminosity of the spark would allow.

It was also noticed during the observations that errors may be introduced by the insensible motions of the eye in front of the slit. With a spectroscope having one flint glass prism of 60° and a telescope magnifying about fifteen times, the displacement of the lines due to this cause as referred to the cross wires was found to amount to as much as forty units, or twice the distance between the magnesium fluting and the less refrangible nitrogen line. With the Cooke spectroscope having eight prisms the displacement was not more than twenty units. Pinholes of various sizes were placed in front of the eyepiece, but the displacement was not at all diminished by this. The motion of the lines over the pointer was found to be quite rhythmical and to keep time with the beating of the heart.

No doubt this displacement could be abolished by perfect focussing, but the construction of instruments generally does not admit of the focussing of the cross wires, and even if there be an adjustment, as there is in the instrument used by me, one condition is only good for one observer.

These experiments, therefore, show that many precautions have to be taken before the coincidence or non-coincidence of one line with another can be determined with absolute certainty even when large dispersion and stable laboratory conditions are employed.

The general results of the laboratory comparisons may be briefly stated thus :—

	Huggins.	Thalén.	Liveing and Dewar (1880).	Liveing and Dewar (1888).	Lockyer.
Mg fluting.......	5006·5	..	5000	5006·4	5006·5
N line...........	5004·6	5005·1	..	..	5005·1
Pb line..........	5004·5	5004·6	..	..	5005·0

It must be remembered that ordinary observatory conditions are not nearly so favourable for accurate measurements of the positions of lines in spectra as laboratory ones. In the first place, the apparatus is not so stable, and must of necessity be in motion, and again, the collimator of the spectroscope with its slit exactly central must be demonstrated to be absolutely in the optic axis of the telescope before a measurement can be taken as final.

Two series of observations should therefore be made, one with the spectroscope in one position, and the other when it has been turned

through 180°. There is no statement in Dr. and Mrs. Huggins's paper that this has been done.

Finally, I may point out that with the above values, and assuming that the nebula line exactly coincides, as Dr. Huggins says that it does, with the least refrangible of the nitrogen double, the difference in position between it and the magnesium fluting is less than a quarter of the distance between the two D lines, and I have shown that this difference may easily arise from instrumental errors.

C. *Observations by a New Method.*

The laboratory work having shown the numerous sources of error connected with observations where great accuracy is attempted, it seemed to me that it was quite hopeless to attempt very accurate observations of nebulæ in the ordinary way, where the conditions are not nearly so favourable as in the laboratory.

I have already pointed out that unless it can be demonstrated that the collimator of the spectroscope is absolutely in the optic axis of the telescope employed, one series of observations alone is worthless. Again, the greater the dispersion employed, the greater generally will be the weight of the spectroscope, and the less the stability of the apparatus. Finally, as the telescope must necessarily be in motion, the conditions are constantly liable to change by the varying dispositions of the various parts of the apparatus.

It struck me that these difficulties could be to a great extent overcome by the use of a siderostat, in which case a spectroscope of any weight could be employed, as it was no longer necessary that it should be in motion. To test this method, arrangements were made for observing the spectrum of the nebula in Orion. A 12-inch siderostat and the 10-inch object-glass of the Science Schools equatorial were employed, in conjunction with an optically perfect Steinheil spectroscope belonging to the Physical Laboratory, and placed at my disposal by Professor Rücker.

The observations commenced on November 27th.

The following account is based upon the records in the note book, further explanatory additions having been made where necessary.

November 27th.—The 10-inch object-glass from the equatorial was supported in a semi-circular wood block, on an adjustable lantern tripod, which was sunk about 6 inches in the ground and the top perfectly levelled. By carefully sighting a lamp supported at the siderostat, the collimator of the spectroscope was placed in a line with it. The object-glass was then put in proper line, and adjusted by observations of Aldebaran with a reflecting eyepiece, which was supported in front of the slit, and so arranged that when an object was in the centre of the field it was also on the slit. Aldebaran was

also used for adjusting the object-glass at the proper distance from the slit. The ordinary cross-wire eyepiece of the spectroscope being replaced by the bright line micrometer, the prisms were adjusted at minimum deviation for λ 500 by observing the spectrum of magnesium ribbon burning in a spirit lamp in front of the centre of the mirror. These preliminaries being completed, the work with the nebula was commenced.

The nebula was first brought upon the slit by means of the reflecting eyepiece, and the observation was attempted with four prisms, but unsuccessfully, as the night was not good and the nebula was low, so two were removed. When this was done, the three principal lines were seen remarkably well, and a very narrow slit could be used. The chief line was made coincident with the illuminated pointer; magnesium ribbon was then burned at the centre of the mirror, and with this dispersion the coincidence between the nebula line and the least refrangible maximum of the magnesium fluting appeared perfect. These observations were made independently by Messrs. Fowler and Baxandall, and Lieutenant Bacon, R.N., temporarily attached to the Science Schools, but in no case was the nebula line seen more refrangible than the magnesium fluting. Another prism was then added, and set to minimum for λ 500.

Absolutely the same result was obtained. The burning magnesium used for comparison was removed from the front of the mirror and placed directly in front of the slit, but still the same result was obtained. The brightness of the nebula lines with three prisms made it evident that another prism might be added.

The magnifying power of the telescope employed was sixteen, and the dispersion C to H with three prisms was 6° 32′.

November 28th.—The observations of the Orion nebula were repeated with similar arrangements to those employed on the previous evening, the fourth prism being now added. The spectrum was very well seen when the nebula was on the slit, but it was very difficult to keep it on, as, in consequence of the looseness of a screw, as it was subsequently found, the siderostat clock worked badly.

One comparison was, however, made by Mr. Fowler, using the same micrometer eyepiece as before and a very narrow slit. The nebula line and the less refrangible maximum of the magnesium fluting were found to be perfectly coincident.

Arrangements had been made during the day for burning magnesium, so as to get parallel rays from it. The method is shown in fig. 2, and consists of a collimator placed in front of the object-glass. When burning the magnesium, a card, with a hole in the centre of the same diameter as the collimating lens, was placed in front of the 10-inch object-glass to keep out stray light. At the spirit lamp end of the tube was a piece of tin foil with a pin-hole at

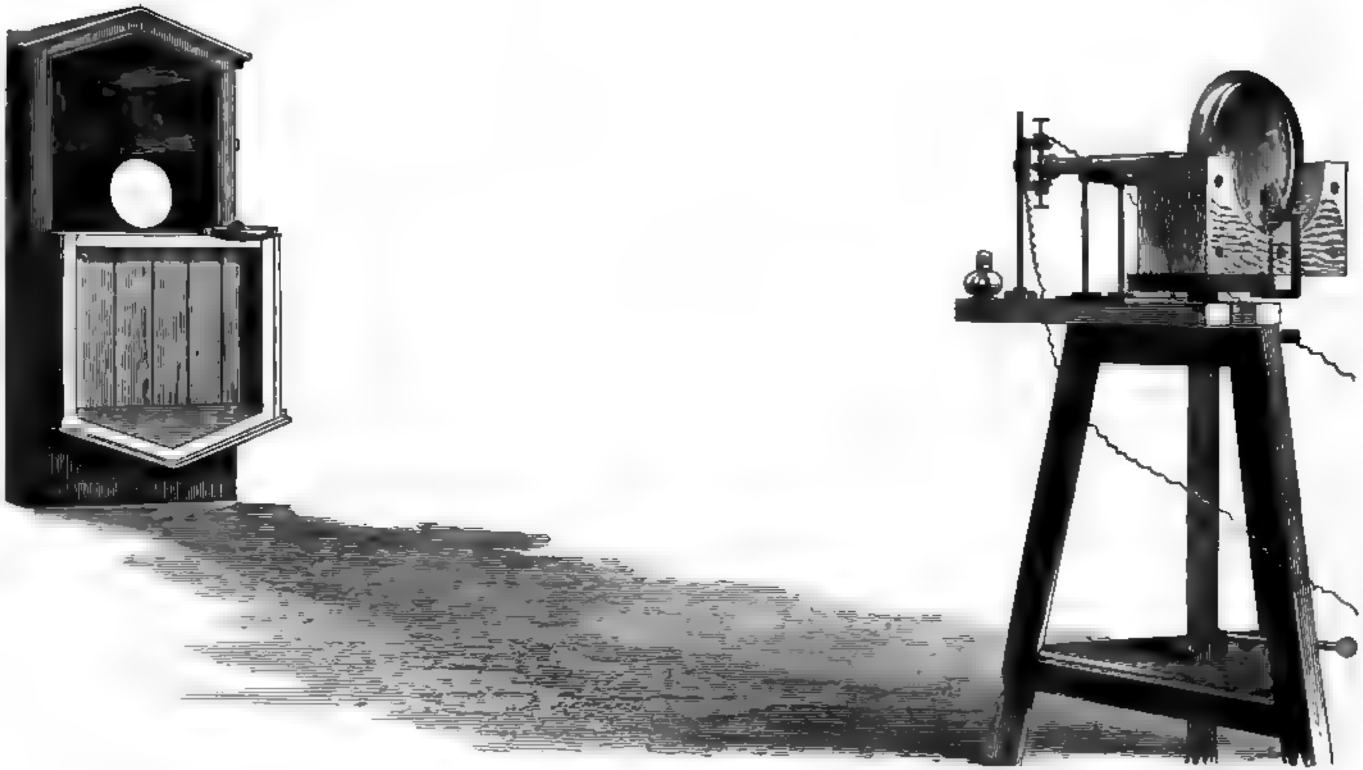


Fig. 2.—Preliminary arrangement of apparatus for observing the spectra of the heavenly bodies by the use of the siderostat. The 10-inch object-glass is supported on an adjustable stand between the siderostat and the spectroscope, the collimator of which is directed accurately to the centre of the mirror. The small collimator between the object-glass and the siderostat is for obtaining comparison spectra either from the flame of a spirit lamp or from the electric spark. The wires leading to the spark-stand pass through an opening in the side of the hut containing the spectroscope and the induction-coil.

the centre. The image of this was focussed on the slit of the Steinheil, and when the magnesium was burning the spectrum was well seen.

November 29th.—During the day the siderostat was put in order.

The position of the collimator of the Steinheil was tested by opening the slit very wide, and burning magnesium at the centre of the siderostat mirror (the mirror being temporarily removed for this purpose). The image of the slit fell exactly in the centre of the collimating lens, so that no alteration was necessary.

An observation was also made of the displacement of the magnesium fluting brought about by moving the spirit lamp in which the magnesium was burned away from the centre of the mirror. It was found that if the lamp were moved more than two inches on either side the spectrum ceased to be visible. Between the two extreme positions (*i.e.*, 2 inches on each side of the centre), the displacement produced was about one-third of the distance between the first and second maxima of the magnesium fluting.

The rough collimator that had been previously used was replaced by the collimator of a student's spectroscope, the slit of which was adjustable both for length and breadth. This was supported on a light iron tripod, so that, when in position, it would prevent very little light from the siderostat passing through the object-glass. By this arrangement the spectra of magnesium and the nebula could be superposed, it being intended to obtain the magnesium fluting in this case from a quantity spark between magnesium poles. An enlarged image of the secondary slit is, of course, formed on the slit of the Steinheil spectroscope.

A complete plan of the apparatus, drawn to scale, and showing some of the principal dimensions, is given in fig. 3. The Steinheil spectroscope employed has a circular table, 28 inches in diameter, supported on a tripod stand weighing about eighty pounds. The collimator and observing telescope are each about 20 inches long, and have object-glasses $1\frac{3}{4}$ inches in diameter. The prisms have faces $2'' \times 2''$, three of them having an angle of 45°, and one of 60°, each one being supported on a stand provided with levelling screws. With the four prisms the dispersion from A to H is 10°.

The reflecting eyepiece which was used to act as a finder was the ordinary one used with the 10-inch equatorial, and could be lifted out of its supports and put back again at pleasure.

No work could be done in the observatory this evening on account of fog.

November 30th.—Commenced work by adjusting the object-glass and the collimator in front of it.

The accuracy of this adjustment was checked by a comparison of *b* the spectrum of the Moon with *b* in the spectrum of magnesium

FIG. 3.—Plan of arrangements for observing the spectra of the heavenly bodies by the aid of a siderostat. A, siderostat; B, 10-inch object-glass and support; C, 4-prism Steinheil spectroscope; D, collimator for obtaining comparison spectra; E, spark stand; F, induction coil; G, reflecting eyepiece to act as finder; H, lamp for illuminating the pointer of the micrometer; K, wall of hut; L, doorway.

burning behind the secondary collimator. There was perfect coincidence between the lines. Four prisms were used for the comparison, and the centre of the Moon's disc was thrown on the slit.

Everything being now in perfect adjustment, the nebula was turned to, and three good comparisons made of the chief line and the magnesium fluting, the magnesium being burned behind the secondary collimator. The pointer was displaced and readjusted in each observation. In each case the coincidence with the least refrangible maximum appeared perfect. These observations were made by Mr. Fowler and Lieutenant Bacon. In order to further test the result obtained, Mr. Fowler put the pointer of the micrometer exactly on the nebula line, and left it for Lieutenant Bacon to say how its position was with respect to magnesium; again it was perfectly coincident.

Lieutenant Bacon made three independent comparisons, the position of the pointer being changed each time, and twice found coincidences with the least refrangible maximum, whilst once the nebula line appeared to be on the right-hand edge of the same maximum. Mr. Gregory also made one comparison which confirmed the above results.

December 1st.—The object-glass having been adjusted by means of Aldebaran as before, the secondary collimator was put in position, and the magnesium spark from a quantity coil put behind the slit. With this arrangement a comparison of the nebula line with the lines of nitrogen could also be made. The collimator was adjusted by means of *b*, as seen in the Moon and in the magnesium spark.

The pointer of the micrometer was then put upon the F line in the Moon, and the nebula was afterwards brought upon the slit. The two lines were coincident, showing the probable accuracy of the adjustments.

Three comparisons were made of the nebula line with magnesium by Mr. Fowler, the pointer being displaced each time; in each case the coincidence was perfect.

Comparisons with the nitrogen lines showed the nebula line to be a little less refrangible.

I made the comparison twice, and in each case the coincidence with the magnesium fluting was perfect. In one case I saw the pointer of the micrometer, the nebula line, and the nitrogen lines at the same time; the pointer was on the nebula line, but both appeared to the right of the nitrogen double.

Finally, Mr. Fowler put the pointer on the nebula line, Lieutenant Bacon agreeing with him as to the setting. Then I made the following comparisons:—

(1.) With the nitrogen lines. Result, pointer to the right.

(2.) Magnesium burning behind slit. Result, coincidence perfect.

(3.) Magnesium burning at the centre of the siderostat. Result, again perfect coincidence.

It was found that magnesium burning at the centre of the siderostat was coincident with magnesium burning behind the secondary slit, thereby showing the accuracy of the adjustments.

In all these observations the nebula line was seen to be sharper on the right-hand edge than on the left, and it was irregularly bright along its length, as in the Westgate observations.

The observations have left no doubt in my mind as to the coincidence of the chief nebula line with the magnesium fluting, under such conditions that at the same time the coincidence of the F line of hydrogen with another nebula line was demonstrated.

Lieutenant Bacon and my assistants concur in this view. Even with four prisms the observations are by no means easy, and are very delicate, but it is important to note that in no observation was the nebula line found more refrangible than the magnesium fluting, and if the optical conditions were imperfect it seems hardly likely that an error in the same direction would be reproduced on four different nights, the apparatus being set up afresh each time.

Arrangements are in progress for repeating the observations with apparatus furnished with screw adjustments.

There is one more test of the accuracy of the adjustments which might have been applied had I thought of it in time. So far the test relied on has been to see that the lines seen in the spectrum of magnesium burning at the centre of the mirror were coincident with those seen when the magnesium was burned behind the secondary slit. In one case the light passed through the object-glass only, while in the other it passed through the object-glass and secondary collimator. When the observations are repeated, it is intended to further test the adjustments by forming an image of the flame at the mirror upon the secondary slit, so that in both cases the light will pass through the collimator and object-glass. If the lines are still coincident, the accuracy of the adjustments will be still further demonstrated.

IV. Fluted Character of the Chief Nebula Line.

Dr. and Mrs. Huggins state that the chief nebula line is perfectly sharp and well-defined. This necessitates my giving in reply a complete account of those recorded observations, which, coupled with my own, have led me to the opposite conclusion, namely, that the line is often noticed ill-defined at the edges, chiefly on the blue side, and in some parts of the nebula in Orion presents even more unmistakable indications that it is the remnant of a fluting.

Certain references in the paper suggest that it may be well that I

should briefly state what I understand by a fluting, and I cannot do this better than by referring to observations of a candle flame which anyone can make. A pocket spectroscope and a lens are all that are needed to follow my remarks. If the image of the base of the flame be projected on to the slit, bright flutings are seen in the green (near *b*), citron, blue, and violet. That in the green is the brightest, and is seen to consist of three apparent bright lines with faint fringes on their more refrangible sides. The different members of the group gradually diminish in brightness, the least refrangible being the brightest. Such a group as this I look upon as a *compound* fluting, and each member itself as a *simple* fluting, since with high dispersion the fringes break up into series of fine lines very close together. If now the image of the flame be gradually raised, so that the base passes off and portions nearer the centre are brought on to the slit, the fainter members of the group gradually disappear, and when a certain point is reached only the brightest, least refrangible, simple fluting is left. This I look upon as the "remnant of a fluting," whether the fluting was in the first instance simple or compound.

The compound fluting of magnesium near 500 is very similar to that of carbon. It consists of a series of bright lines of gradually diminishing brightness and increasing distances apart towards the more refrangible end, and each has a fringe on the more refrangible side. The first maximum (the least refrangible) is brighter than the others, and the fringe close to it is brighter than the second maximum, and so when "the remnant of the magnesium fluting near 500" is referred to, the first maximum with that portion of its fringe which is brighter than the second maximum is meant.

Before I give the observations of the character of the chief nebula line in historical sequence, I quote Dr. Huggins's statement:—"My own observations of the line, since my discovery of it in 1864, with different spectroscopes up to a dispersion equal to eight prisms of 60°, show the line to become narrow as the slit is made narrow, and to be sharply and perfectly defined at both edges."

The following are the first recorded observations:—

In 1864, the spectrum of the Dumb-bell nebula in Vulpecula was observed, and it was noted* that the light of this nebula, after "passing through the prisms, remained concentrated in a bright line, corresponding to the brightest of the three lines represented in fig. 5, Plate X. *This line appeared nebulous at the edges.*"

Similarly, it was recorded in 1866 of the spectrum of General Catalogue No. 4403:—†"The spectrum of this nebula indicates that it posesses a gaseous constitution. One bright line only was

* 'Phil. Trans.,' 1864, p. 441.

† 'Phil. Trans.,' 1866, p. 385.

seen, occupying in the spectrum apparently the same position as the brightest of the lines of nitrogen. *When the slit was made as narrow as the intensity of light would permit, this bright line was not so well-defined as the corresponding line in some of the other nebulæ under similar conditions of slit, but remained nebulous at the edges.*" An observation of the spectrum of General Catalogue No. 4572,* also made in 1864, led the observer to record :—"The spectrum of this nebula consisted of one bright *nebulous* line of the same refrangibility as the brightest of the lines of nitrogen," and in the same paper we read :—"One bright line only was distinctly seen, of apparently the same refrangibility as the brightest of the nitrogen lines. *This bright line appeared by glimpses to be double.* Possibly this appearance was due to the presence near it of a second line."

These observations show conclusively that the chief nebula line has not always been described as "sharply and perfectly defined at both edges," to use Dr. Huggins's language of 1889, and to no one should this fact be more manifest than to Dr. Huggins, since the above observations were made by him.

Secchi, one of the first observers of nebular and stellar spectra, in observations of some planetary nebulæ made in 1866,† saw the three principal lines, and noted that "the planetary nebula in Andromeda has the lines above named, but the principal one is a little diffused."

The observation relating to the presence of a second bright line very near to the chief nebula line might have been of considerable importance, and have afforded an almost crucial test of the validity of my identification of the line. This second line might well have been the second maximum of the magnesium fluting, but Dr. Huggins's statement as to its position is so loose as to make it impossible for me to say whether such is the case or not.

I should, however, have been unjustified in relying upon Dr. Huggins alone; and it will be seen from what follows that nearly all observers of nebula spectra have noted at some time or other that the chief nebula line appeared undefined at one edge, as if it were part of a fluting.

In 1871 Vogel made some observations of the spectra of nebulæ.‡ I have noted that in 1864 Dr. Huggins observed only one bright line in the spectrum of the Dumb-bell nebula, and recorded this line as *nebulous* at the edges. Vogel's observations of the spectrum of the same nebula in 1871 agree, as regards the character of the line, in every respect with that of Dr. Huggins.

The following is Vogel's description :—"Sehr heller grosser Nebel der unter dem Namen Dumb-bell bekannt ist. Das Spectrum desselben

* 'Phil. Trans.,' 1866, p. 386.

† 'Bullettino Meteorologico,' 31st Oct., 1866.

‡ 'Bothk. Beob.,' Leipzig, Heft 1, 1872, p. 56.

wurde am 21 Mai, 1871, untersucht, es besteht aus zwei Linien, von denen die erste mit der Stickstofflinie (Wellenlänge 500·4 Milliontel Millimeter) coincidirt; diese Linie erscheint aber hier breiter als in den Spectren der planetarischen Nebel *und ist besonders nach dem violetten Ende des Spectrums* SEHR VERWASCHEN."

Here, then, it is again explicitly stated that the nebula line was considerably ill-defined on the violet edge.

Bredichin made a series of observations of the three nebula lines in 1877, and he noted also that the chief line was less defined on the blue edge. In the words of this observer, "se presentait comme une bande, une peu plus claire vers le rouge."*

The following is the description of the chief nebula line, as seen in the Orion nebula, given by Mr. Maunder in 1884:—†

"The line λ 5005 was examined with this latter dispersion (two-prism train), the slit being very narrow, and was seen to be a single line. None of the lines in the spectrum of the nebula are, however, very sharp. λ 5005 *showed a faint fringe mainly on the side nearer the blue.*"

It must be borne in mind that as these observations of the undefined condition of the blue edge of the nebular line were made before special attention had been directed to it by my paper of November, 1887, they were *absolutely unbiassed.* Prior to 1887, no one had suggested that the line might be the remnant of a fluting. Indeed, Dr. Huggins contended for a line of an unknown form of nitrogen.

I have already quoted Dr. Huggins's present declaration, that since his discovery of the nebula line in 1864 he had always observed it as sharply and perfectly defined at both edges, by which assertion he practically repudiates his own published observations. But Dr. Huggins has done more than this; he has put himself to the trouble of communicating with other observers of nebula spectra with a view of obtaining their opinions as to the character of the chief line. I need only refer to Dr. Huggins's correspondence with Professor Vogel, who wrote,‡ in answer to a letter from him and in support of his view:—"Beeile ich mich Ihnen mitzutheilen, dass meine langjährigen Beobachtungen über die Spectra der Gas-Nebel vollkommen mit den Ihrigen darin übereinstimmen, dass die Nebellinie λ 5004 schmal, scharf und NICHT VERWASCHEN IST." With reference to the observation of Dr. Vogel as to the undefined character of one edge of the nebula line, previously referred to in this reply, Dr. Huggins remarks as follows:—"In an early observation of the Dumb-bell nebula, Professor Vogel, indeed ('Beobachtungen zu Bothkamp,' p. 59, 1872), describes this line as less defined towards the violet side. In a letter

* 'Annales de l'Obs. de Moscou,' vol. 3, 1877, p. 120.

† 'Greenwich Spectroscopic Results,' 1884, p. 5.

‡ 'Roy. Soc. Proc.,' vol. 46, p. 53.

(April 3, 1889), Professor Vogel says this appearance of the line was probably due to a slit not sufficiently narrow. He says that he re-examined this line in his observations with the great Vienna refractor, and that it did not then appear otherwise than defined and narrow."

I fancy that Dr. Huggins and Professor Vogel must know that widening the slit does not generally cause a well-defined line to become less defined *on one side only*. Again, the fluting would very probably be seen little better with the Vienna refractor than with that at Bothkamp; for I find that the brightness of the nebula in the former is to that in the latter only about as 13 to 10.

Since my paper of November, 1887, was written other observers besides Dr. and Mrs. Huggins have had their attention directed to nebular spectra, with special reference to the character of the chief line.

My first observations of the nebula of Orion from this point of view were made at Westgate-on-Sea in October, 1888, by means of a 12-inch mirror that had been kindly placed at my disposal by Mr. Common. The image of the nebula being allowed to float slowly over the slit, I distinctly got the impression that the line in question varied in its behaviour from the other lines, and that at the points where it was brightest it extended most towards the blue end of the spectrum. The observations were repeated at Kensington with the 10-inch equatorial by Mr. Fowler, Demonstrator of Astronomy, and Mr. Baxandall, and they arrived at the conclusion that the chief line had a decidedly fluted appearance.

This observation is further borne out by Mr. Taylor, who, referring to an observation made in November, 1888,* states:—

"The 5001 line is by far the brightest in the spectrum. It is never seen sharp, but, with the narrowest slit, always has a fluffy appearance, this being much more marked on the blue than on the red edge. This was most carefully examined for evidence of structure, but the line was always found to be single, and no decided evidence of fluting structure could be made out." It is clear from this observation that the line fades away towards the violet end of the spectrum, although the actual compound structure of the magnesium fluting is not visible. I shall presently have to refer to an experiment which shows that the compound structure would not be likely to be visible.

I have quite recently (October 29) observed the spectrum of the nebula in Orion with my 30-inch reflector at Westgate-on-Sea, using an enlarged form of pocket spectroscope with a dispersion which does not split D, and the observation is, to my mind, final. I found that in certain parts of the nebula the lines were knotted, and in others broken; but in the former case, whilst the F line thickened

* 'Monthly Notices, R.A.S.,' vol. 49, p. 125.

equally on both sides, the chief line thickened only on the more refrangible side. This result is shown in fig. 4.

FIG. 4.—Diagram showing the appearance of the three principal lines in the spectrum of the nebula in Orion as observed in the Westgate 30-inch reflector.

This was confirmed by Messrs. Fowler and Baxandall at Kensington, with the 10-inch equatorial on October 31st and November 1st, and again by Mr. Fowler, with the 30-inch, on November 2nd. It may be noted also that I got momentary glimpses of many bright lines between F and G on October 31st. Messrs. Fowler and Coppen have since made some very careful observations of the Ring nebula in Lyra, and also record the chief line as having a fringe on the more refrangible side. A line less refrangible than 500 in the neighbourhood of *b* was suspected; this may turn out to be the carbon fluting near 517: the absence of the hydrogen line in the 10-inch was important as indicating that the nebula is in an advanced stage of condensation, approaching that of the nebula in Andromeda.

In the observations with the siderostat arrangement, as pointed out in the extracts from the observatory note-book, the chief line was noted by Mr. Fowler and Lieutenant Bacon to have a decided fringe on the more refrangible side.

It may be remarked that high dispersion is not so likely to show the fluted character of the chief line as low, for the more the fringe is dispersed the fainter it must become.

In consequence of the brilliancy of the Orion nebula, the fluted appearance of the chief line would be more manifest than in any other nebulæ, and the absence of the fringe when the line is seen in the spectra of fainter nebulæ is therefore not antagonistic to the view that the line may be the remnant of the magnesium fluting. This must not be misinterpreted. Given two nebulæ, exactly alike in every respect but temperature, then the line, if visible in both, would appear more like a compound fluting in that nebula of which the temperature

was lower, and would become more like a line as the temperature was increased.

But this is not all; a greater number of collisions per unit volume at the same temperature would increase the visibility of the effects, and greater brightness in a nebula may proceed from this cause as well as from a less distance. We should not, therefore, expect to see the fluting, even if its existence be conceded, in all cases, and the smaller the dispersion the better it will be seen, *cæteris paribus*. Experiments have been made here on the spectrum of magnesium when seen very faintly with moderate dispersion.

The conditions being such that the structure of the fluting near 500 was well visible when magnesium ribbon was burned in front of the slit, a sufficient thickness of neutral tint glass was introduced to reduce the brightness of the fluting until it was about equal to that of the chief line seen in the spectrum of the nebula in Orion. Under these conditions, the 500 fluting is only faintly visible and the secondary maxima entirely disappear. We get only the brightest, least refrangible member of the compound fluting, together with a simple fringe of light without structure on the more refrangible side. This experiment was shown at the Royal Society Conversazione in May, 1889, and a note upon it may be found on page 13 of the programme. The experiment has recently been repeated and fully confirmed with a four-prism Steinheil spectroscope. It was found best to adjust the dark glasses so that two or three of the maxima were seen when the magnesium was burning; then, when the magnesium was just dying out, only the least refrangible one, with a slight fringe, was seen.

The greater luminosity of the first maximum and its fringe has also been observed in another way. Magnesia, volatilised in the oxy-hydrogen flame, with the proportion of gases properly adjusted, gives the compound fluting pretty bright. If, then, the quantity of hydrogen be increased or diminished gradually, whilst the oxygen remains constant, the fluting gradually disappears, but the first maximum and its fringe are seen when all the others have disappeared.

Numerous photographs have also been obtained which show the first maximum brighter than any of the secondary ones.

These experiments not only show that the first maximum is brighter than the secondary ones, but further, that some of the fringe on the more refrangible side of it is also brighter. In observations of nebulæ, therefore, if the chief line be due to magnesium only a very slight fringe would be observed unless the luminosity be sufficient to render visible some of the secondary maxima.

I have shown, therefore, that many records exist as to the fluted appearance of the chief nebula line—records that amply justify the

identification of it with the low-temperature magnesium fluting near 500, an origin that seemed most probable from my experiments on the spectra of meteorites. The fact that one or two published observations have now been practically withdrawn does not affect the main issue in the faintest degree.

Whatever the chemical origin of the line, the historical statement I have just given affords good grounds for believing that it is certainly a remnant of a fluting.

V. Conclusion.

The facts recorded in this paper seem to me to demonstrate conclusively that the line under discussion is due as the induction suggested to magnesium.

High dispersion has been employed, and we now know that the line seen in the meteoritic glows is truly the remnant of the magnesium fluting. We further know that the nebula line is coincident with the edge of the magnesium fluting when the two are compared with a four-prism spectroscope and a high magnifying power, both nebula and magnesium being observed under absolutely the same conditions. Even if we accept Dr. Huggins's observation of 1868, the nebula line only differs in position from the magnesium fluting by a quarter of the distance between the D lines, and we know that many sources of error may explain that difference.

Finally, many observations, both new and old, show that the nebula line resembles the first maximum of the magnesium fluting in having a fringe on its more refrangible side, and I have shown that the spectrum of magnesium may be observed under such conditions that only the first maximum and its fringe are visible.

The discussion of the other lines is reserved for a further communication, as the work connected with them is not yet completed.

With regard to the concluding part of Dr. and Mrs. Huggins's paper, I have recently sent in communications to the Royal Society from which it will be gathered how independent the meteoritic hypothesis is of the visible radiation of magnesium in meteorites at the temperature of nebulæ. But whether the line referred to in this paper be due to magnesium or not, I am glad to find that Dr. Huggins has so far accepted the views which I have recently put forward as to admit in the paper under reply that the nebulæ may "represent an early stage in the evolutionary changes of the heavenly bodies," and that they may stand at or near the beginning of the evolutionary cycle so far as we can know it;* whereas he formerly held that "the nebulæ which give a gaseous spectrum are systems possessing a structure, and a purpose in relation to

* 'Roy. Soc. Proc.,' vol. 46, p. 59.

the universe, altogether distinct and of another order from the group of cosmical bodies to which our Sun and the fixed stars belong;"* and that: "We have in these objects to do no longer with a special modification only of our own type of Suns, but find ourselves in the presence of objects possessing a peculiar and distinct plan of structure."† I shall take a subsequent opportunity of showing how untenable is the view he now communicates, that, although the nebulæ represent early evolutionary forms, they are at a high temperature and that the constituents of the mass are arranged in the order of their vapour densities. I refrain from discussing these points on the present occasion; but I may remark that if such a view were true, and we further accept the statements that the nebula line was seen in the comets of 1866–67 and that Nova Cygni probably exists as a planetary nebula of small angular diameter, we are driven to the conclusion that comets reduce their temperature as they approach the Sun, and that "new stars" get hotter as their luminosity diminishes.

"Note on the Spectrum of the Nebula of Orion." By J. NORMAN LOCKYER, F.R.S. Received and read February 13, 1890.

In a former communication I gave in detail observations made by means of a siderostat, which seemed to put beyond all reasonable doubt the question of the origin and true wave-length of the chief nebula line. Although, as I stated in the communication referred to, I regard this question as one of secondary importance, I have commenced another series of investigations with a view of eliminating all possible instrumental errors. The new method has not been completely carried out, but a sufficient approximation to it has been reached to render the results obtained of some interest.

Using the siderostat, object-glass, and collimator as before described, the method in question consists in using a vacuum tube, giving the lines both of hydrogen and nitrogen in front of the slit of the collimator. The tube made for this purpose was found to have leaked when there was an opportunity of using it, so that the observations of hydrogen and nitrogen, in comparison with the nebula lines, have not been made in the same field of view at the same time. The hydrogen tube and an air spark with iron poles (iron poles being chosen in order to check the position of the nebula line near λ 495) were, however, placed alternately in front of the slit of the collimator, and this enabled the observations to be made with almost equal accuracy. I give the following extract from the Observatory

* 'Roy. Soc. Proc.,' vol. 14, p. 42.

† 'Phil. Trans.,' 1864, p. 442.

note-book. The observations were made by Mr. Fowler (who was assisted by Mr. Coppen) on February 5:—

"Made further observations of the nebula of Orion with 4-prism Steinheil spectroscope. First compared nebula spectrum with spectrum of spark between iron poles close to slit, the secondary collimator not being used at all.

"*Results.*—495 nebula line exactly coincident with iron line 4956·8. At the same time, the 500 nebula line was certainly less refrangible than the nitrogen lines.

"Next adjusted collimator and put hydrogen tube and iron spark successively in front of slit.

"*Results.*—3rd nebula line coincident with F line of hydrogen.

"495 line coincident with iron 4956·8.

"500 line less refrangible than nitrogen lines.

"500 line exactly coincident with magnesium fluting, whether the magnesium was burned behind the slit of collimator or at the centre of siderostat mirror."

It will be seen that these observations entirely confirm those which I have already communicated to the Society, and also carry the work a step further in the determination of the actual wave-length of the nebula line near λ 495 by the siderostat and collimator method.

"Preliminary Note on Photographs of the Spectrum of the Nebula in Orion." By J. NORMAN LOCKYER, F.R.S. Received and read February 13, 1890.

In other communications to the Society, I have shown that the chief nebula line coincides absolutely in position with the remnant of the fluting seen in the flame of burning magnesium near λ 500, with the highest dispersion we could command at South Kensington. Attempts have recently been made, therefore, with the 30-inch reflector at Westgate-on-Sea, to obtain photographs of the spectrum of the nebula, using magnesium as the term of comparison. The objects sought were, primarily, to determine whether there was a line in the nebula corresponding with one of the lines of the magnesium triplet about λ 373, and to obtain as complete a photographic record as possible of the spectrum between this triplet and λ 500. With this view, Mawson's instantaneous plates were used, these having been found to be fairly sensitive to the green. The exposures have been carried up to four hours, and five photographs have already been taken, some of them with shorter exposures than that named, in consequence of the sky becoming clouded or irregularities in the driving clock, which is not yet completely finished. One plate only was exposed for four hours, on February 11, but, unfortunately, in

consequence of the high wind, the slit was covered for an unknown part of this time by the velvet used to keep out stray light, and this was not at once discovered, as the finder for directing the telescope is at the lower end of the reflector tube, away from the spectroscope. This photograph only shows three or four of the more prominent lines, but they are all sharply defined. The other photographs were taken on February 2, 8, 9, and 10, the last with an exposure of three hours.

As a collimator has not yet been fitted to the tube of the reflector, the exposure of the plate to the flame of burning magnesium was made by closing the mirror cover, and burning magnesium at its exact centre. One half of the slit was exposed to the nebula, and the other half to the magnesium.

Two prisms of 60° were employed. The part of the nebula photographed was the bright portion preceding the Trapezium. In some cases, in consequence of clock irregularities, the stars of the Trapezium have imprinted their spectra upon the plates, but these in no way interfere with the spectrum of the nebula, since a longish slit was used, and the spectra of the stars are narrow.

There is a remarkable and almost absolute similarity between the photographs obtained. The best one, taken on February 10, shows all the lines of the other photographs in addition to others, and this has therefore been selected for the determination of wave-lengths; it contains at least twenty-eight lines, about eight of them falling between F and G.

The principal lines are the three ordinarily seen in the visible spectrum, the lines of hydrogen at G, *h*, and H, and the strong line in the ultra-violet near λ 373. G is by far the strongest line in the spectrum. The wave-length of the least refrangible line on the photograph was taken as 5006·5, as determined at Kensington, and this, together with the hydrogen lines and the ultra-violet magnesium triplet in the comparison spectrum, formed the basis of the curve for determining the positions of the fainter lines.

The lines next in importance to those already mentioned are near wave-lengths 4470, 3890, and 3870. The first of these, the strongest between F and G, is probably the line observed by Dr. Copeland, and, as I have stated in a previous paper, is possibly Lorenzoni's *f* of the chromosphere spectrum. There are also two fairly obvious lines between F and 495.

Amongst the fainter lines, the most prominent are near λ 4027 and 4045, the former doubtless being the strong fine triplet seen in the flame spectrum of manganese.

Other still fainter lines are also shown, amongst which the most interesting are the flame lines of calcium near 3933 (K) and 4226, and lines near 4690 and 4735, which are probably the boundaries of

the compound carbon fluting. It seems, therefore, probable that all the fainter lines are either due to carbon or to low-temperature metallic lines.

It is a very striking fact that some of the chief lines are apparently coincident, although the statement is made with reserve, with the chief bright lines in P Cygni, a magnificent photograph of which I owe to the kindness of Professor Pickering; it is one of the Henry Draper Memorial photographs.

The wave-length of the line about λ 373 may, perhaps, even yet not be considered quite settled; but this much may be said, that, in those photographs in which the chief nebula line is sensibly coincident with the magnesium fluting, the ultra-violet line is very nearly, if not quite, coincident with the least refrangible member of the magnesium triplet (λ 3730). This, however, is somewhat uncertain, because of the over-exposure of the magnesium spectrum. It is certainly not coincident with either of the more refrangible lines of the triplet, as the measured distance between the two lines of the nebula is almost the same as that between the least refrangible line and the fluting near λ 500 of the magnesium spectrum.

The most satisfactory determination of the position of the ultra-violet line has been made by a comparison of the two photographs of February 10 and 11. The magnesium spectrum in the latter photograph is more clearly defined than in the former one, the slit being narrower, and the other instrumental conditions remaining the same.

The distance between the fluting near 500 and the least refrangible member of the triplet on the photograph of February 11 was found to be very slightly less than that between the two nebula lines on the photograph of February 10. According to these measures, the nebula line falls between the two magnesium lines at 3730 and 3724, about one-sixth of the distance between them from the former. giving its wave-length as nearly 3729. These measures, however, must only be regarded as preliminary.

A complete map is being prepared by Mr. Fowler, but, as it requires careful manipulation of the incident light for the detection of the more delicate lines, it is not yet completed. I have asked Mr. Fowler to take complete charge of this work, for the reason that the sensitiveness of my own eyes is somewhat impaired.

I have finally to express my great obligations to Mr. Fowler for the zeal and patience which he has displayed in taking the photographs. He is entirely responsible for those taken on February 2, 10, and 11, when I was away from Westgate.

"On a Re-determination of the Principal Line in the Spectrum of the Nebula in Orion, and on the Character of the Line." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received March 20,—Read June 12, 1890.

We think it desirable to put on record the results of a re-determination of the position of the principal line in the spectrum of the nebula in Orion, under the more favourable conditions of a higher position of the nebula, and of some improvements in the instrumental arrangements.* The spectroscopes have been furnished with new and sensibly perfect object-glasses by Sir Howard Grubb, and a new bright pointer has been fitted to the spectroscopes by Mr. Hilger, which is illuminated by a small incandescent lamp, of which the brightness is controlled by suitable resistances. In all other respects the instrumental arrangements have remained unaltered. The same spectroscope, giving a dispersion of about four prisms, which was described in my paper of 1872 as Spectroscope B,† and was used in the work on this line contained in my paper of 1874,‡ and also throughout the work of last year, with the exception of one single confirmatory observation with a more powerful spectroscope,§ was employed in the present investigation, and also the same arrangements for the comparison spectrum from burning magnesium.

In my earlier spectroscopic work I pointed out that a possible parallactic error of the comparison spectrum may easily come in when a small reflecting prism is placed in the usual way before one half of the slit; and also the possibility of errors from the unavoidable flexure of the spectroscope or of its attachments to the telescope. In 1872, I adopted the plan of placing "the spark or vacuum-tube within the telescope at a moderate distance from the slit. For this purpose holes were drilled in the telescope-tube, opposite to each other, at a distance of 2 feet 6 inches within the principal focus. Tubes were fixed by screws over these holes, and in these tubes slide suitable holders for

* [In a communication last January to the Royal Society, Professor Lockyer stated that he and his assistants had by different methods and with great dispersion compared directly the chief line in the spectrum of the Nebula in Orion with the band of the magnesium-flame spectrum, and that they had found perfect coincidence between the nebular line and the terminal line of the band. Professor Lockyer also stated that they had always seen the line as a fluting. These statements being in direct contradiction to my early observations and to the conclusions of our paper of last year, the necessity was thrown upon us of going over our work again.—July 4.]

† 'Roy. Soc. Proc.,' vol. 20, 1872, p. 382.

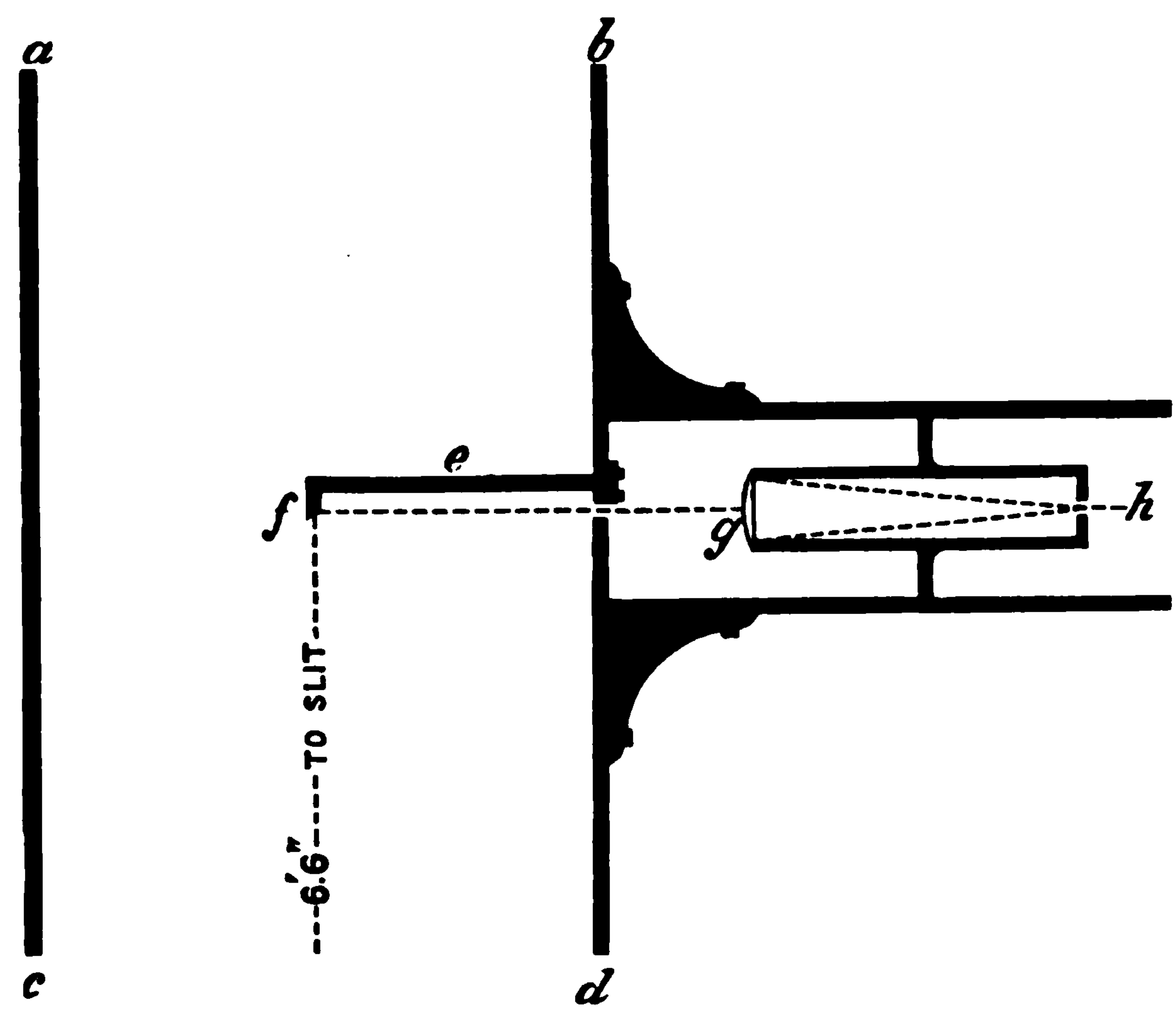
‡ 'Roy. Soc. Proc.,' vol. 22, 1874, p. 252.

§ 'Roy. Soc. Proc.,' vol. 46, 1889, pp. 50, 51.

carrying electrodes or vacuum-tubes. The final adjustment was tested by the comparison of the bright lines of magnesium and the double line of sodium with the Fraunhofer lines *b* and D in the spectrum of the Moon."*

I have since adopted an arrangement in which, when once adjusted, any sensible parallactic effect from a change of position of the source of light seems to be impossible, for even a minute motion of the spark or other source of light for comparison has the effect of throwing the light to one side, without the slit; so that, as long as the comparison spectrum is seen, there can be no doubt that the direction of the light for comparison, as it fell upon the slit, had remained invariably the same, relatively to the optical axis of the telescope, and consequently to the celestial spectrum under observation.

In the diagram, *abcd* represents a section of the telescope-tube near the middle of its length; within this is firmly screwed a thin



steel arm, *e*, carrying a minute mirror, *f*. This mirror is about a quarter of an inch in width, and of about the same apparent length, when seen fore-shortened from the slit. The mirror is fixed at a distance of 6 feet 6 inches within the principal focus, where the slit is placed. In the side of the tube opposite the face of the mirror is a small hole, through which the light from the collimator *g* passes on to the mirror. At the other end of the collimator, which has a length of about 7 inches, is a diaphragm with a small hole, *h*, before which the source of light, whether an induction spark, a vacuum-tube, or

* 'Roy. Soc. Proc.,' vol. 20, p. 382.

burning magnesium, is placed. The lens at g is so placed as to bring the light approximately to focus at the place of the slit.

It is obvious that with this arrangement an extremely small shift of the light before the hole h would be sufficient to cause the ray reflected from the mirror to go off the slit, and that the reflected light can pass into the slit only so long as its direction remains sensibly invariable relatively to the optical axis of the telescope. It is also obvious that any flexure in the spectroscope, or in the tube connecting it to the telescope, would affect similarly the light from the nebula and from the magnesium. The precaution was taken, however, to so orient the spectroscope, that any flexure from the weight of the instrument would be in the direction of the length of the slit.

The coincidence or otherwise of the direction of the light reflected from the little mirror with the optical axis of the telescope can be determined by comparing the spectrum of burning magnesium with b in the spectrum of the Moon, or in that of the light of the sky. As an additional safeguard in the comparison of the spectrum of the nebula with magnesium, since my early observations had shown the nebular line to be very slightly more refrangible, the mirror was purposely so adjusted that, though the lines of the burning magnesium were seen to fall upon the corresponding dark lines b in the Moon or sky, yet a careful observation would show a very minute overlapping of the bright lines towards the blue. This state of things would diminish a little the interval which should be seen between the nebular line, and the termination of the magnesium-flame band and so make the observation more difficult. It is evident that if under such circumstances of adjustment the nebular line were seen on the more refrangible side of the magnesia band, the observation, being a delicate one, would be more trustworthy, for in the case of coincidence with magnesium the line would appear towards the opposite and less refrangible side of the magnesia line, broadening the line towards the red (*loc. cit.*, p. 49).

The stability of this adjustment depends upon the rigidity of position of the little mirror within the telescope; as this weighs only a small fraction of an ounce, and is supported by a strong steel arm firmly attached by four screws to the steel tube of the telescope, there is an almost complete absence of any chance of its displacement. During twelve months not the smallest alteration has been detected, though very careful examinations have been frequently made.

At the time the comparisons were made last year, namely, in March, the nebula was getting low, and from perhaps an excess of caution I described them as follows: "Although I consider the results to be satisfactory, I prefer to say that I and Mrs. Huggins, independently, believed fully at the time that we saw the appearance which all former observations of the line led me to expect, namely,

the nebular line to fall within the termination of the magnesium band" (*loc. cit.*, p. 49).

This year the position of the nebular line within the termination of the magnesia band has been confirmed by both of us independently on several nights.

The more refrangible position of the nebular line relatively to the termination line of the MgO band has been ascertained not only by repeated comparisons of the two spectra by means of a suitably illuminated pointer, but also this year, as last year, by occasional moments of direct vision of the nebular line upon and within the magnesia band. It is only occasionally that the necessary relative brightness of the band can be secured, but such moments of direct vision of the two spectra are very trustworthy.

On February 9th, Professor Liveing made some observations on the spectrum of the nebula, and I have his permission to quote from the notes which he entered at the time in my observatory book. During the afternoon he examined the adjustments of the little mirror. His words are: "Observed in Dr. Huggins' spectroscope attached to his telescope the Fraunhofer lines *b*, as given by the clouds, and the bright lines of burning magnesium thrown in by reflexion. The solar spectrum was but faint, so that it was necessary to use rather a wide slit. I observed a close coincidence between the dark lines of the sky light and the bright lines of the burning magnesium; the two overlapped, but the dark lines extended a very little on the less refrangible side, the brightest line a very little on the more refrangible side beyond the dark line."

In the evening he observed the nebula, and recorded his observations in the following words:—"Observed the spectrum of the nebula in Orion, and compared the position of the least refrangible line with the magnesia fluting. The latter was thrown in by reflection from burning magnesium. I put the nebular line on the pointer first, and then from time to time the magnesium was burnt. I made quite sure that the edge of the magnesia fluting was less refrangible than the nebular line; repeated the observations several times. Tried to see both the nebular line and the fluting at the same time, but found it hard to see both at once, but I still came to the same conclusion, namely, that the edge of the fluting was less refrangible than the nebular line."

Afterwards, Professor Liveing observed the third line of the nebula, together with $H\beta$ from a vacuum-tube. He says:—"Compared the position of the most refrangible of the nebular lines with the F line of hydrogen thrown in by reflexion from a vacuum-tube, the coincidence seemed perfect, the one line falling upon the other."*

* [On April 29, Professor Liveing was kind enough to go over again with us the arrangements for the comparison spectrum, and, in particular, to see if any error

We have since gone further, and attempted a quantitative estimation of the distance of the nebular line within the termination of the band. For this purpose we made use of the minute apparent breadth of the illuminated pointer-tip as a measuring unit. The value of this unit was determined by measuring with it the distance of b_3 from b_4 in the solar spectrum.

Independent estimations made by both of us on several occasions agreed in assigning to this distance, after taking into account the minute displacement of the comparison-spectrum by the little mirror towards the blue,

A wave-length of about .... λ 0001·5.

Deducting this distance from λ 5006·5, the position of the termination of the band, we get for the nebular line

A position of about .... λ 5005·0.

At the time of these observations the earth's motion caused the nebular line to be degraded towards the red by about λ 0000·25. If, therefore, the Great Nebula has no motion of its own, this interval must be deducted from the observed position of the nebular line,

Placing it at about.............. λ 5004·75.

The observations recorded in the paper of 1874* gave the position of the nebular line relatively to the fiducial lead line with an accuracy not less than λ 0000·5. This relative position was translated into wave-lengths in our paper on the Nebula (*loc. cit.*, p. 45), showing that the nebular line lies from about λ 5004·6 to about λ 5004·8.

The question whether this nebula has a motion in the line of sight

could possibly arise from a change of position of the magnesium during its burning. After a detailed account of the experiments, he wrote in my note-book :—" I could not detect any shift ; and I came to the conclusion that there is no sensible shift due to moving the burning magnesium. I next compared the position of the lead line near the edge of the MgO band, as seen in the same spectroscope detached from the telescope, with the said edge of the band. Both could be seen at the same time, and the apparent distance between them was so great that even if there should be some shift of the lines from the method of throwing in the light when the spectroscope is attached to the telescope, I am satisfied that it could not amount to anything comparable with the distance between the lead line and the edge of the MgO fluting. So far as my memory will serve, the distance from the edge of the MgO fluting at which the nebular line appeared when I observed it on February 9 was not far short of the distance now observed between the lead line and the edge of the MgO fluting."—July 4.]

* 'Roy. Soc. Proc.,' vol. 22, 1874, p. 251. This paper claims for the determination of the position of this line in the case of seven nebulæ an accuracy sufficiently great to show a motion of 25 miles per second. This motion corresponds to about λ 0000·67, but as some of the nebulæ were more difficult to observe than the bright nebula in Orion, the accuracy of the determination of the line in this nebula may certainly be taken as not less than the amount given in the text, namely, λ 0000·5.

must be determined by comparisons of the third line with the corresponding bright line of a hydrogen vacuum-tube. The observations I recorded in 1874, as well as those of Mr. Maunder, of Greenwich (*loc. cit.*, p. 60), "show that the nebula has very little, if any, sensible motion in the line of sight."

The direct comparison was made on several nights with results similar to the observation that Professor Liveing recorded on February 9, namely, that "the coincidence seemed perfect, the one line falling upon the other."

We have endeavoured to push this observation further, to determine if the coincidence was absolute, or whether there was a very minute overlapping of the edges of the two lines. The adjustment of the apparatus would throw the hydrogen line, to a very minute extent, towards the blue, at the same time that the earth's motion would degrade the nebular line from the hydrogen line towards the red.

The faintness of the third line with a narrow slit does not permit us to speak with absolute certainty as to the extent which the hydrogen seemed to overlap the nebular line towards the blue.

We were quite certain that the hydrogen line did overlap the nebula slightly towards the blue, but we were unable to determine whether the overlapping corresponded accurately to the earth's motion at the time of observation. It appeared to do so approximately, which would support my former conclusion, that the "nebula has very little, if any, sensible motion in the line of sight."

PART II.

On the Character of the Principal Line in the Spectrum of the Nebula in Orion.

In our paper last year (*loc. cit.*, p. 53) I stated that "my own observations of this line, since my discovery of it in 1864 . . . . . show the line to become narrow as the slit is made narrow, and to be sharply and perfectly defined at both edges." We gave also the corroborative evidence of two accurate observers who have made a special study of the spectrum of the gaseous nebulæ, Professor Vogel and Dr. Copeland.

Since last year the defining power of the spectroscopes has been improved by two new object-glasses by Sir Howard Grubb. The nebular line has been subjected on several nights to a very searching examination with different widths of slit; and with different magnifying powers on two spectroscopes—the one with a single prism of 60°, the other, the "four-prism" spectroscope (*loc. cit.*, p 49).

We came to the conclusion that a marked feature of this line is its sharply-defined character on the more refrangible side; we were

unable, under any of the conditions of observing, to detect even a suspicion of any softening of the more refrangible edge of the line, much less the faintest indication of a "flare," and certainly not the distinctive peculiarity of a "fluting."

In the case of observations with small dispersion, the eye is helped by placing the second line, which then appears near the first, behind a bar fixed in the eye-piece.

Observations of the nebula in Orion by eye, as well as the photographs of Mr. Common and of Mr. Roberts, show numerous small irregularities in the brightness of the nebula, which give rise to a closely-mottled appearance. As the length of the slit takes in a considerable angular extent of nebula, several of these irregularities of brightness or "mottlings" are usually included within it, giving to the nebular lines an irregularly bright or blotchy appearance. As the nebula is allowed to pass over the slit this blotchy appearance is seen to vary in the size and in the number of the brighter patches, and also in their brightness relatively to the less luminous spaces between them. At the first glance, in some positions of the slit upon the nebula, the lines, and especially the principal line as the brightest, appear almost as if serrated at the edges. A little attention soon shows that this is a purely physiological effect due to the greater brightness of the patches, and that the brighter parts of the line do not really project beyond the less brilliant intervals between them. One marked character of this phenomenon is that both edges of the lines appear equally serrated, and that there is no indication of a spreading of the brighter patches towards the blue only. It is easily ascertained that this more or less patchy condition is not peculiar to the principal line, for precisely the same patches can be detected in the other two lines, and the patches can be seen to correspond in number and in position within the lines.

These observations, repeated on several nights, have left no doubt in our minds that the principal line is certainly as sharp and as bright on the side towards the blue as on the less refrangible side.

On February 9, Professor Liveing scrutinised the character of this line. His words are:—"Observed the nebular line with various widths of slit. The line always appeared sharply defined on the more refrangible side, whether the slit were wide or narrow. On gradually closing the slit, the line fined down to a very fine line. The same appearance as to sharpness on the more refrangible side was observable with a spectroscope of less dispersive power and with eye-pieces of low power as with the higher dispersion and greater magnification."

The observations recorded in this paper appear to us to show conclusively:—

(1.) That the principal line is not coincident with, but falls within, the termination of the magnesium-flame band.*

(2.) That in the nebula of Orion this line presents no appearance of being a "fluting."

It is scarcely needful to say that, in the face of the observations recorded in this paper, we are not able to accept the conclusions arrived at by Professor Lockyer in his recent communications† to the Royal Society. From them it would appear that Professor Lockyer confirms my statement made in 1874,‡ that the second line "is sensibly coincident with an iron wave-length 4957" (Thalén, λ 4956·8; Liveing and Dewar, λ 4956·9); and also that Professor Lockyer's photographs confirm my photographs of 1882, 1888, and 1889, in that it is a single strong line, and not a triplet, which appears in the ultra-violet region, and that this strong line is more refrangible than the first component of the magnesic oxide triplet.§

Addendum. Received June 6, 1890.

1. *Addendum on the Position of the Line.*

One of the planetary nebulæ, in the spectra of which I found in my earlier comparisons with lead|| in 1874, that the principal line had sensibly the same position as the corresponding line in the nebula of Orion was Σ. 5 (G. C. 4234). We have now compared again the principal line in this nebula with the lead line λ 5004·5 with the same spectroscope (spectroscope B, 3rd eye-piece) and an arrangement for

* [Even if the nebular line appeared to be sensibly coincident under the amount of dispersion which can be brought to bear upon the nebulæ, for reasons stated in our paper of last year (*loc. cit.*, p. 55, foot-note), the evidence would be strongly in favour of the view that the coincidence was apparent only, and against the assumption that the nebular line was to be regarded as the "remnant" of the magnesium-flame band. We did not, however, give sufficient prominence to the fact of the great brilliancy of the line in many nebulæ, without the faintest traces of the second and third flutings. The relative intensities of the brightest ends of these flutings are:—

1st fluting	.. ..	8
2nd „	.. ..	7
3rd „	.. ..	5

(Watts, 'Index of Spectra,' p. 175). However, the position of the nebular line at a measurable distance from the terminal line of the magnesium-flame band towards the blue makes such considerations superfluous, and disposes finally of the assertion that the nebular line is the "remnant" of the magnesium fluting.—July 4.]

† 'Roy. Soc. Proc.,' vol. 47, p. 129 and p. 189, &c.

‡ 'Roy. Soc. Proc.,' vol. 22, p. 252.

§ 'Roy. Soc. Proc.,' vol. 46, p. 54.

|| 'Roy. Soc. Proc.,' vol. 22, 1874, p. 254.

the comparison spectrum similar to that described in the first part of this paper, but in which the small mirror has been replaced by a very small total reflecting prism. The correctness of position of the comparison spectrum was ascertained by repeated comparisons of the bright lines of magnesium at *b* with the corresponding dark lines in the Sun's light reflected from the sky.

When in this spectroscope the spectrum of lead is observed together with that of burning magnesium, the lead line is seen to fall well within, and to be separated by a clear space from, the terminal line of the magnesium-flame fluting.

The principal line of Σ. 5, like that of the nebula of Orion, appears when the slit is made narrow to be very thin and clearly defined at both edges. The lead line is a thin and defined line; if, therefore, the nebular line were coincident with the terminal line of the magnesium-flame fluting, it would appear in the spectroscope to be separated by a clear space from the lead line towards the red. As the angular diameter of the nebula is small, the line is much shorter than the lead line—not longer than about one-third of the height of the spectrum, and consequently its position relatively to the lead line, even when it falls partly upon it, can be very accurately determined.

The nebular line was seen as a short thin bright line partly upon, and partly clinging to, the lead line. The nebular line in our instrument certainly fell upon the lead line, but overlapped it a very little, though not so much as by half its breadth, on the less refrangible side. This position agrees precisely with that described in my early observations made nearly twenty years ago, when I employed for the first time lead as a fiducial comparison line.* As I stated in 1874,† "if greater prism power could be brought to bear upon the nebulæ, the line in the lead spectrum would be found to be in a small degree more refrangible than the line in the nebula;" and, of course, if sufficient power of dispersion were employed, the nebular line would be seen separated from the lead line towards the red, and not, as in our instrument, partly upon the lead line.

These observations, both those by myself in 1874 and the recent observations made by both of us independently on four different nights, place the nebular line exactly where it was ound to be by our direct comparisons with burning magnesium in the nebula of Orion (which were confirmed by Professor Liveing), namely, as not coincident with, but as falling well within, the terminal line of the magnesium-flame spectrum.

It should be stated that on two nights we made comparisons of Σ. 5 with burning magnesium, both directly, and indirectly by means of the illuminated pointer. The observations completely confirmed

* 'Roy. Soc. Proc.,' vol. 22, p. 252.

† *Ibid.*

the results of the lead comparisons, which were, however, more easily made, as it is difficult to see the exact position of the short nebular line when it is upon the bright fluting.*

2. *Addendum on the Character of the Line.*

I am permitted by Dr. Copeland, Professor Young, and Mr. Keeler, of the Lick Observatory, to quote the following observations, which they have been so kind as to make at my request, of the character of the principal line in the spectrum of the Great Nebula in Orion.

Dr. Copeland writes, dated March 26, 1890: "I find it difficult to make anything satisfactory of nebular spectra with my present apparatus, working in the smoke of Edinburgh . . . . . On the 14th I saw the three lines as well as I am likely to see them until we get to work at the new observatory. All the lines were just as broad as the slit; when the slit was wide open they were broad, and when the slit was closed slowly they gradually became narrower and narrower."

Professor Young, writing on March 21, 1890, says: "I have not been able this winter to try the observations for wave-length, having no convenience for the comparison spectrum, but I have carefully examined the spectrum of the nebula of Orion, both with a heavy glass prism, and with a grating of 14,000 to the inch, and a collimator of 16 inches focus. With the prism the brightest nebular line seemed absolutely *sharp*, and cleanly defined on both sides; with the grating the line was fainter, and I could not use so narrow a slit, the dispersion was much higher also; the line therefore was a little hazy, *but equally so on both sides.*"

At the Lick Observatory there was a continuance of bad weather during the early months of the year, but Mr. Keeler, with Dr. Holden's kind permission, observed the nebula on two nights. He observed successively with one prism, a powerful compound prism, and then with a Rowland grating, 14,000 + lines to the inch. With this grating, the collimator was 20 inches in length, the observing telescope 10½ inches, with an eye-piece magnifying 13·3 times. The slit was narrow, 0·0025 inch. The spectra up to the fourth order were employed.

Mr. Keeler says: "One thing that struck me particularly, and that there could be no doubt of, was the perfect sharpness and

* [On one night, as had been frequently done when we were observing the nebula in Orion, after the comparison of the nebula with lead and magnesium had been made, the spectroscope was left attached to the telescope in order that we might verify the correctness of position of the comparison spectrum by means of the light of sky on the following day, without any change whatever having taken place in the adjustments of the instrument. The result of this verification was, as always when working on Orion, absolutely satisfactory.—July 4.]

fineness of the nebular lines under the very considerable dispersion used. There is not the least doubt in my mind that they are all of gaseous origin—not 'remnants of flutings.'"

[The observations with large dispersion by Professor Young, and especially those of Mr. Keeler, after observing with one prism, and then with a compound prism, that the line remained sharp even when examined in the 4th spectrum of a grating 14,000 + to the inch, are of great value in regard to Mr. Maunder's observations. It was on one occasion only when he made use of the very great dispersion of 80° from A to H, equal to about sixteen prisms of 60°, that he observed the nebular line to be otherwise than sharp and defined. On this unique occasion he says: "The three lines were seen as narrow bright lines, but none of them were perfectly sharp; each showed a slight raggedness at both edges, but in the case of the line near λ 5005 it was clear that this fringe or raggedness was more developed towards the blue." Mr. Maunder significantly adds: "In the case of the other two lines they were not bright enough for it to be possible to ascertain whether the fringes were symmetrical or not."* The new observations at Princeton and at the Lick Observatory would seem to leave little doubt that if the other lines had been as bright as the principal line, the raggedness about them would have been found to be equally unsymmetrical, and that the want of symmetry affected all three lines, and was probably instrumental.—July 4.]

Second Addendum. Received July 4.

On the Position of the Line.

On account of the unusual weather at the Lick Observatory during the early part of this year, Dr. Holden informs me that "The observing chances have been amazingly small." For this reason, although, in addition to the observations on the character of the chief line in the nebula of Orion, measurements of its position were attempted on two nights, the interruption from clouds was so constant that they could not be satisfactorily completed.

Under these circumstances, I asked Dr. Holden to have the kindness to telegraph to me if Mr. Keeler should be able to confirm the position of the line as not coincident with the magnesium-flame fluting in the nebula Σ. 5.

On June 15th, I received a telegram with the words: "Confirmed Struve. 5. Keeler."

I have received since a letter from Mr. Keeler, dated June 14th, 1890, in which he says:—"Last night I compared the brightest line

* 'Monthly Notices, R. A. S.,' vol. 49, 1889, p. 308.

in the spectrum of Σ. 5 with the magnesium fluting of nearly the same wave-length, and I am glad to say that my observations were in accordance with your own." . . . . "On comparing the brightest line with the magnesium fluting, both directly and by aid of the micrometer wire, the line was seen to be well within the limits of the fluting, and separated by a small but unmistakable interval from its bright lower edge. The appearance was the same on both sides of the grating, and in the 3rd and 4th spectra. The comparison apparatus was carefully adjusted, and no shifting of the line was caused by changing the position of the spark. The edge of the fluting *could not* be brought into coincidence with the nebular line. No measurement of the difference of wave-length was made, as my attention was directed to the main fact of the non-coincidence of the line in all positions of the instrument. I will make such measures as soon as possible."

"Note on the Photographic Spectrum of the Great Nebula in Orion." By William Huggins, D.C.L., LL.D., F.R.S., and Mrs. Huggins. Received April 16,—Read June 12, 1890.

From an examination of the photographs of the spectrum of the nebula in Orion taken by us in 1882, 1888, and 1889, we suggested in our paper "On the Spectrum, Visible and Photographic, of the Great Nebula in Orion,"* "that the mottled and broken-up character of the nebular matter shown in Lord Rosse's drawings from eye-observations, and much more strikingly brought out in the recent photographs of Mr. Common and of Mr. Roberts, may be connected with differences of spectrum in the photographic region, though in the visible region there is no known alteration of the spectrum of the four bright lines, except it may be some small differences of relative brilliancy of the lines. Until next winter we cannot go beyond the new information which these photographs give to us."

Unfortunately, the necessity thrown upon us of making a laborious redetermination of the position and character of the principal line in the visible spectrum,† which has confirmed in every point the results contained in our paper of last year (*loc. cit.*), has deprived us of the more favourable opportunities during the past season of carrying out our intention of photographing the spectra of different parts of the nebula.

We have obtained two photographs only, one taken on March 14th and 15th, and the other on March 17th; but these suggest how much

* 'Roy. Soc. Proc.,' vol. 46, p. 42.

† 'Roy. Soc. Proc.,' vol. 47, pp. 129 and 189, &c.

information a spectroscopic examination by photography of the nebula in detail would probably give to us.

These photographs, taken of almost the same part of the nebula as the photograph of 1889, showed, to our surprise, the lines of hydrogen at *h* and at H strongly impressed upon the plate, though these lines were carefully searched for in vain in our former photographs; in them no trace of these lines could be detected, but the line near G was strong, and there was present a large number of faint lines, of about thirty of which the approximate measures were given in our paper.

The new photographs show not only the lines of hydrogen at *h* and H, but also the first two lines of the ultra-violet series in the white stars which I described in 1879.* Four of these lines had been photographed in the spectrum of hydrogen by Dr. H. W. Vogel, in 1879, and the entire series, with the exception of one, has been since obtained by Cornu in exceptionally pure hydrogen.†

The line α at λ 3887·8 is strong, and the next line β at λ 3834·5, though much fainter, is certainly present. There is evidence of light-action on the plate at the position of the line γ which we believe to be present; and we suspect, from traces of photographic action, that one or more of the other lines of the white star series might have come out with a longer exposure.

It is not necessary to point out in the present note the importance of the presence of these more refrangible lines of hydrogen in respect of the view we have to take of the condition of things in the nebula. In this connexion it is significant that the hydrogen lines are sensibly stronger and broader on the plate as the Trapezium with its stars is approached.

Between the hydrogen lines α and β there is a line stronger even than α, which has a wave-length of about λ 3868.

We do not find any line in the photograph exactly at the place of the solar line K; the position of this line appears to correspond to a gap between two lines on the plate. We suspect the broad line on the less refrangible side of the place of K would probably be resolved by a narrower slit into two or more lines.

The strong line which was first seen in our photograph of the nebula taken in 1882 is certainly stronger than Hγ, and is by far the most powerful line in the photographic region. On account of the wide slit employed in my original photograph, I put the line at about λ 3730; from measures of the line in a photograph taken in 1889, with a narrower slit, we found that its position was more refrangible, and we gave the approximate wave-length "about λ 3724." There was necessarily some difficulty in determining its position

* 'Phil. Trans.,' 1880, p. 669.

† 'Journal de Physique,' 2nd ser., vol. 5, Aug., 1886.

exactly on account of the small scale on which, from the faintness of the light of the nebula, it is desirable with the telescope at our disposal to take the photographs, and also because in the nebular spectrum itself we had no fiducial line nearer than Hγ. In the photographs taken this year we have the advantage of the known position of the hydrogen line at H, and with the help of this line our recent measures show that the "about" must be interpreted as slightly less refrangible than λ 3724. Without attempting to fix its position absolutely, we believe that the line will be found to fall between λ 3725 and λ 3726. It is not needful to point out that measures of these little photographs cannot compare in accuracy with direct comparisons with considerable dispersion, as in the case of our observations of the chief line of the nebula by eye. It is, however, now certain that the line does not coincide with any one of the three components of the magnesic oxide triplet, but is less refrangible than the middle line at λ 3724, and falls between this line and the first line of the triplet at λ 3730.

In these photographs there is a strong line, besides many faint lines, on the less refrangible side of G.

The background of the spectrum is seen to contain numerous faint lines, which, as far as we have been able to identify them, are the same as those seen in our earlier photographs, of some of which approximate measures were given in our paper, but they are, possibly on account of a slightly wider slit, not so easily measured as they were in the former photographs, in which no traces of the hydrogen lines at *h* and at H could be detected.

A marked feature of the lines consists of their abruptly different intensities at different parts of their length, giving the blotchy appearance which is characteristic of the lines in the visible spectrum, and which we have described in our recent paper "On a Redetermination of the Position and Character of the Principal Line in the Spectrum of the Nebula in Orion." The length of the slit takes in a large angular extent of the nebula, and, therefore, usually includes within it one or more of the brighter "mottlings" which are so well shown in photographs of the nebula. It is to be remarked that these brighter blotches are sharply bounded, showing that the different parts of the nebula are to some extent distinct and often become suddenly brighter than the neighbouring parts.

The lines of the new photographs contain two very strong and abruptly-bounded blotches, and a third one less marked.

These brighter blotches, corresponding to different conditions of closely-adjacent nebular matter, give an explanation of an appearance which we recorded last year in speaking of the strong line "about λ 3724." "On one side of the star-spectra this line is a little broader than on the other side; but, as a similar appearance is presented by

Hγ and the stronger lines of the group, it may arise from some optical or photographic cause."*

We now learn that this difference between two parts of the lines indicates probably a different condition of the nebula on the two sides of the star-spectra.

Other lines besides those described in this note are present, not only between G and F, but also on the more refrangible side of the strong line about λ 3725.

The importance of the new points which have come out from these photographs makes us regret that we must postpone a fuller examination and discussion of the spectrum of different parts of the nebula until its return next year.

"On a new Group of Lines in the Photographic Spectrum of Sirius." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received April 25,—Read June 12, 1890.

In 1879,† I gave an account of a series of broad lines in the photographic region of the spectrum which was found to be characteristic of Sirius, Vega, and other white stars, and which was identified as a continuation of the spectrum of hydrogen beyond H.‡ In the photographs of Sirius which I had taken up to that time, I was not able to be certain if the two most refrangible of the lines, θ and ι, were present. This uncertainty has been set at rest by photographs taken since, in which the complete series of the hydrogen lines, including θ and ι, come out with great distinctness.

I have long suspected the presence of another group of broad lines some distance further on in the ultra-violet region, but until this year we have not been able to see them in the photographs with sufficient distinctness to be able to make even roughly approximate measures of their positions.

On April 4th, a photograph of the spectrum of Sirius was taken with a long exposure, the slit being made very narrow, in the hope of bringing out this new group of lines with greater distinctness. This plate shows, on examination, that the spectrum of Sirius, after the termination of the hydrogen series, remains, as far as we can see at present, free from any strong lines until a position as far in the ultraviolet as about λ 3338 is reached, at which place appears the first of a group of at least six lines, all nearly as broad as those of the hydrogen series. The third line of the group about λ 3278 appears to be

* 'Roy. Soc. Proc.,' vol. 46, p. 54.

† 'Phil. Trans.,' 1880, p. 669.

‡ H. W. Vogel, 'Berlin, Akad. Monatsber.,' 1879, July 10; and Cornu, 'Journal de Physique,' 2nd ser., vol. 5, 1836, p. 100.

the broadest, but they are all broad, though even in this photograph they are not seen with the distinctness which is necessary for ascertaining accurately their relative character.

The sixth line occurs where the spectrum is faint, almost at the limit of this photograph, which was taken when Sirius was some distance past the meridian, and we are not able to find out whether this line completes the group, or whether there may not be other lines still more refrangible belonging to it. We expect to be able to determine this point, namely, whether the group ends with the sixth line, when the opportunity comes round of being able to photograph the star when it is near the meridian.

The new group of six lines is well seen when the photograph is examined with a lens, but when the plate is placed under the measuring microscope it is only with some difficulty that the lines can be observed with the distinctness which is necessary for measuring them with a fair approach to accuracy.

For this reason, the wave-lengths given below must be regarded as only preliminary, and but roughly approximate measures of the positions of the new lines.

1st Line............	λ 3338
2nd ,, ...	λ 3311
3rd ,, ...	λ 3278
4th ,, ...	λ 3254
5th ,, ...	λ 3226
6th ,, ...	λ 3199

"On the Spectra of Comet *a* 1890 and the Nebula G.C. 4058." By J. NORMAN LOCKYER, F.R.S. Received and Read June 12, 1890.

The comet discovered by Mr. Brooks on the 19th of March (*a*, 1890) has recently been observed at Kensington with the view of testing the sequence of spectra which resulted from my discussion of all the spectroscopic observations of comets which had been made up to the end of 1888.* The orbit, however, is such that the comet has only passed through a small range of temperature, and no changes have been observed in its spectrum beyond the gradually increasing brilliancy of the carbon bands relatively to the continuous spectrum. As I pointed out in the paper referred to, the citron band should be most variable, for the reason that the brightest flutings in the spectra of manganese and lead fall near it; but, although this band has been carefully observed on every occasion, it has retained the same wave-

* 'Roy. Soc. Proc.,' vol. 45, p. 189.

length, and was found by direct comparisons to be coincident with the carbon band near λ 564. From this point of view, therefore, the observations have not supplied us with any new facts, and, as the comet is calculated to have passed perihelion on June 3, it is not likely that any phenomena will be seen beyond the usual diminution in brightness of the carbon flutings.

The observations which have been made, however, showed such a striking similarity between the spectrum of the comet and that of the Nebula in Andromeda that advantage was taken of its presence to compare its spectrum with that of an adjacent nebula, G. C. 4058 (R. A. 15 h. 3 m. 24 s., Decl. + 56° 11′), which closely resembles the Andromeda Nebula.

In my paper of January, 1889 (p. 216), I referred to some observations of the spectrum of the Nebula in Andromeda which were suggested by the discussion of cometary spectra. These observations showed that the spectrum of the Nebula was really a cometary one; that is to say, that it consisted of the chief bands of carbon, with a slight modification of the citron band. This result was first obtained by Mr. Fowler in November, 1888, and was subsequently confirmed by Mr. Taylor. The bright bands are superposed on a continuous spectrum which is not much fainter, and they had consequently escaped observation until specially looked for. The other nebula with which a comparison has been made is comparatively near to the comet, and, as its spectrum strongly resembles that of the Nebula in Andromeda, it affords a good opportunity for observing the similarity.

The following extracts from the Observatory note-book will show how close the resemblance is, and I communicate this note to the Society in order that anyone possessing a telescope with an aperture of 10 inches or more may see for himself while the comet is with us that we are dealing with the radiation of carbon vapour in one case as well as in the other:—

May 21.—The comet was something like the Nebula in Andromeda in general appearance, with the exception of the elongation. It was whitish and round, but brightened in the middle to an ill-defined nucleus. No further structure could be seen. With the direct-vision spectroscope, a moderately bright continuous spectrum was visible, with bright flutings superposed. The flutings extended from the nucleus to the boundaries of the comet, but they were very faint, except in the nucleus itself, and there they were not very clearly defined on account of the continuous spectrum. Made several direct comparisons with the blue base of a spirit-lamp flame by means of the microscopic glass reflector in front of the slit. The three flutings in the spectrum of the comet were quite coincident with those in the flame. The fluting near λ 517 was much brighter than the other two,

and the blue band was brighter than the citron. The latter was the most difficult to measure. The hydrocarbon band near λ 431 was not visible. The continuous spectrum extended from about D to a little beyond λ 474.

May 22.—Nucleus of comet not nearly so central as last night. Formation of tail apparently commencing. No very obvious change in the spectrum. The fluting at 517 was possibly a little brighter, but there was no change in the citron fluting. The flutings still extended over the whole comet. Still a fair amount of continuous spectrum, to a large extent masking the carbon flutings.

May 23.—Form and spectrum of comet not distinguishable from last night. The nucleus is possibly still less central.

May 27.—The appearance and spectrum of the comet were precisely the same as on May 23. It seemed a little fainter, but this was probably due to moonlight and a slight haze.

June 6.—The comet was much brighter than on previous occasions. The tail was longer and was turned from the Sun. There was less continuous spectrum than before, so that the bright flutings were more distinct, and a narrower slit could be used for comparisons. The coincidences with the carbon flutings were still quite perfect with the dispersion employed. There was no shift in the citron band.

June 9.—The comet did not appear any fainter, notwithstanding the published ephemeris. There was still less continuous spectrum than on June 6. The 517 fluting now very clearly seen. No change in the positions of the bands.

G. C. 4058.—This is a white nebula in Draco, apparently somewhat similar to the Andromeda Nebula. The spectrum, according to Dr. Huggins, is continuous. The observations showed the spectrum to be irregularly continuous, and remarkably similar in appearance to the spectrum of Comet Brooks. The brightest part was found by direct comparison to be coincident with the carbon fluting λ 517. No definite measures were made of any other brightnesses, but 517 is certainly present, and is not much less distinct than in the spectrum of the comet. The fluting appeared relatively a little brighter than in the Andromeda Nebula. The length of continuous spectrum was about equal to that of the comet.

All the observations were made with the 10-inch refractor by Mr. Fowler. The similarity of the spectra of the comet and the nebula on June 9 was confirmed by Mr. Baxandall.

The observations emphasise what I pointed out to the Society two years ago, namely, that the spectra of nebulæ and comets are similar both at aphelion and during the approach to perihelion. The facts being so, we are justified in considering, therefore, that nebulæ which present to us a carbon spectrum are more condensed than those which

show the line at λ 500 first observed in a comet by Dr. Huggins in the year 1866. It remains for those who hold that the physical structure and temperature of comets and nebulæ are not similar in each case to explain the phenomena observed in a more simple and sufficient way.

June 19, 1890.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

The Rev. J. Kerr, Prof. W. H. Perkin, jun., Mr. D. Sharp, and Mr. W. F. R. Weldon were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Determination of some Boiling and Freezing Points by means of the Platinum Thermometer." By E. H. GRIFFITHS, M.A., Sidney Sussex College, Cambridge. Communicated by R. T. GLAZEBROOK, F.R.S. Received May 27, 1890.

(Abstract.)

The paper contains an account of experiments made with a view of—

(I.) Comparing the closeness of agreement between the readings of platinum thermometers made from different samples of wire arranged in different manners and insulated by different materials.

(II.) Ascertaining some accurate method of graduating such thermometers without the direct use of the air thermometer.

(III.) The determination of certain boiling and freezing points.

Eight thermometers were constructed. The resistance of the platinum coils used varied from about 4 to 50 ohms. Full particulars of these thermometers are given in the paper.

The insulating substance in thermometers A, B, C, D was glass only; but the internal arrangements were different, as also were the samples of platinum wire used in their construction.

The form finally adopted (that of E, F, G) was as follows:—A coil of fine platinum wire was wound on a roll of asbestos paper and slipped into a thin hard-glass tube. Thick platinum wires ran from

this coil to the top of the instrument, and the unimmersed portion of the stem was surrounded by the outer tube of a condenser, and kept at a constant temperature by a flow of tap-water. The resistance of this stem was so small that the change in resistance caused by the changes in the temperature of the tap-water might be neglected.

The diameter of these thermometers was less than $\frac{3}{16}$ of an inch, and their length about 18 inches. They were extremely sensitive, and could therefore be used to trace the rise in temperature due to suffusion, the freezing points of the metals experimented upon being determined by the limit of this rise.

These thermometers were graduated by the temperature of the boiling points of water, naphthalene, benzophenone, and sulphur, and the freezing point of water.

The values obtained by Crafts ('Paris, Soc. Chim. Bull.,' vol. 39) were used in the case of naphthalene and benzophenone, and Regnault's value of the boiling point of sulphur.* The purity of the samples was ascertained by fractional distillation and by the temperature of the melting points. The selection of these temperatures was forced upon me by the results of my experiments, and the reasons for their adoption are fully given in the paper.

The results were plotted in the manner suggested by Callendar ('Phil. Trans.,' A, 1887), and on a scale such that a difference of 0·02° could be read with certainty.

The curves thus obtained differed considerably from each other and from the curve given by Callendar.† However, intermediate temperatures deduced from these curves showed remarkably close agreement.

In no case (the total number of experiments exceeds 300) is the divergence of any one experiment from the mean value obtained from all the thermometers as great as 0·2°, and if only the results obtained from thermometers E, F, and G be taken, the divergence is in no case as great as 0·05°.

The chief difficulties which presented themselves were—

(*a.*) Variations in the resistance of the connexions between the thermometer coil and the resistance coils.

(*b.*) Variations in the temperature of the resistance coils themselves.

(*c.*) The rise in temperature of the thermometer coil due to the current used when measuring its resistance.

* Boiling point of naphthalene (760 m.m.) = 218°·06.
 „ benzophenone „ = 306°·08.
 „ sulphur „ = 448°·34.

† A discussion of the probable cause of these divergences is given in an appendix to the paper.

Table II.

I. Nature of experiment.	II. Thermometer used.	III. Number of determinations.	IV. Mean temperature.	V. Extreme divergence of any experiment.	VI. Temperature adopted.*	Previous observers.
B. p. of aniline (760 mm.)— Sample I	F.	6	184·23	+·03° −·01	184·22	Ramsay, 184·41. Thorpe, 183·7.
	E.	3	184·19	+·02 −·01		
Sample II	E.	4	184·21	+·03 +·02		
	G.	2	184·23	±·00		
B. p. of methyl salicylate (760 mm.)	F.	1	223·21	—	223·19	Ramsay, 222·88. Cahours, 222.
	E.	3	223·18	+·03 −·03		
B. p. of triphenyl methane (770·8 mm.)	E.	1	357·38	—	357·35	Kekulé, 355 (760). Crafts, 358 (754).
	G.	1	357·33	—		
B. p. of mercury (760 mm.)	F.	2	357·68	±·00	357·65	Regnault, 357·25. Ramsay, 358·2.
	E.	4	357·64	+·02 −·01		
	G.	2	357·62	±·00		
Freezing point of tin	F.	3	232·00	+·02 −·01	232·03	Reimsdyk, 228·5. Kupffer, 230. Person, 232·7. Crichton, 238.
	G.	4	232·02	±·02		
	E.	2	232·08	±·00		

Freezing point of bismuth— Sample I.	E.	3	269·68	+ ·02 / − ·01	269·68	Person, 270·5. Reimsdyk, 268·3.
	G.	3	269·69	± ·01		
Sample II (Johnson and Matthey.)	E.	3	269·69	+ ·02 / − ·04		
	G.	3	269·68	+ ·02 / − ·01		
Freezing point of cadmium— Sample I. (Harrington Bros.)	E.	3	321·51	+ ·01 / − ·00	321·67	Person, 320·7. Reimsdyk. 320. Van der Wyde, 325.
	G.	2	321·49	+ ·02 / − ·01		
Sample II (Johnson and Matthey.)	E.	2	321·70	± ·02		
	G.	3	321·64	± ·02		
Freezing point of lead— Sample I. (Harrington Bros.)	F.	3	328·27	+ ·02 / − ·01	328·78	Person, 326·2. Kupffer, 334. Quincke, 330.
Sample II (Johnson and Matthey.)	G.	3	328·79	+ ·00 / − ·01		
	E.	3	328·77	± 02		
Freezing point of zinc	G.	3	421·21	± ·01	421·23	Reimsdyk, 420. Wright and Luff, 420. Person, 433·3. (Other observers range from 342 (Daniell) to 450 (Boussingault).
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In no case would the divergence shown in Column V affect the fourth figure in Column VI.

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(*d.*) The presence of currents due to thermal effects.

(*e.*) Superheating during distillation, and radiation from the source of heat to the thermometer.

(*f.*) The changes in boiling points due to changes in the barometer.

(*g.*) Oxidation of the metals when fluid.

For an account of the manner in which these difficulties were overcome, and of the further precautions taken to secure accuracy, reference must be made to the paper.

The boiling points of the following substances were determined :—Aniline, methyl salicylate, triphenylmethane, and mercury; and the freezing points of tin, bismuth, cadmium, lead, and zinc.§ Every endeavour was made to secure pure specimens of these bodies.

Full particulars of the individual experiments are given in the tables attached to the paper, and the results are summarised in the accompanying tables.

In Table I, I give certain boiling points as determined by means of thermometers A, B, C, D, and E*. I have thought it unnecessary to give details in Table I, since the forms of thermometers used therein were ultimately discarded in favour of the form adopted in E, F, and G. The mean results, however, are in close agreement with those given in Table II (see pp. 222—223).

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Thermometers used.	A.	B.	C.	D.	E*.	Mean.
B. p. of aniline (760 mm.)	184·32	184·27	184·29	184·21	184·24	184·27
„ methyl salicylate „	223·08	223·12	223·16	..	..	223·12
„ mercury „	357·61	357·59	357·65	357·54	..	357·60

In Table II, I give the results obtained from thermometers E, F, and G, together with the extreme departure of any single determination from the mean.

In case the values of the fixed points assumed when graduating these thermometers are hereafter found to be inaccurate, sufficient data are given in the tables attached to the paper for the correction of the temperatures given in column VI.

The results given bear out the following conclusions :—

I. That although the curves of platinum temperature obtained

§ The apparatus used for melting these metals, and for stirring them when cooling, was kindly placed at my disposal by Messrs. Neville and Heycock, and is described by them in their paper on the Melting Point of Alloys ('Journal of the Chemical Society,' May, 1890).

from different thermometers vary considerably, intermediate temperatures deduced from these curves are in practical agreement.

II. That thermometers made and graduated as described may be used for the accurate determination of temperatures up to about 500° C.

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(Abstract.)

The experiments of Helmholtz and Piotrowski* on the oscillations of a metal sphere suspended bifilarly, and filled with various liquids, gave finite values to the slipping coefficients. The inside of the sphere was gilded and polished, and the value obtained for the coefficient λ was, in the case of distilled water, 2·3534 mm. From some experiments of Girard† on transpiration through copper tubes, Helmholtz deduces the value $\lambda = 0·3984$ mm. for water flowing past a copper surface.

In treatises on hydrodynamics, it is shown that when the motion through a tube is linear, the flux is

$$\frac{1}{8}\frac{\pi r^4}{\rho\mu}\frac{p_1-p_2}{l}+\frac{1}{2}\frac{\pi r^3}{\beta}\frac{p_1-p_2}{l},$$

or

$$\frac{1}{8}\frac{\pi(p_1-p_2)}{\mu\rho l}\left\{r^4+4\mu\rho\frac{1}{\beta}r^3\right\}.$$

In Helmholtz's notation this becomes (ρ being taken as unity)

$$\frac{1}{8}\frac{\pi(p_1-p_2)}{\mu l}\{r^4+4\lambda r^3\}.$$

Putting $r = 0·05$ and $\lambda = 0·23534$, we get

$$\frac{1}{8}\frac{\pi(p_1-p_2)}{\mu l}\times 117·67\times 10^{-6};$$

whereas if there is no slip, so that λ vanishes, the flux becomes

$$\frac{1}{8}\frac{\pi(p_1-p_2)}{\mu l}\times 6·25\times 10^{-6}.$$

* 'Sitzungsberichte der Wiener Akademie,' vol. 40.

† 'Mémoires de l'Institut,' 1813—1815.

Thus the flow through a gilt tube of a millimetre in diameter should be twenty times as fast as through a tube where there is no slip. Poiseuille showed that for a glass tube $\lambda = 0$, and it had been generally supposed that this also held for other substances wetted by water.

Such a large effect as the above shows that the existence of the coefficient would be much better investigated by transpiration experiments than by oscillating spheres, and an investigation has been carried out on these lines.

In order to prevent absolute determinations, the time of flow of a known volume of water through a glass tube was observed, the interior of the tube silvered, and another observation taken with the same pressure and the same volume of water.

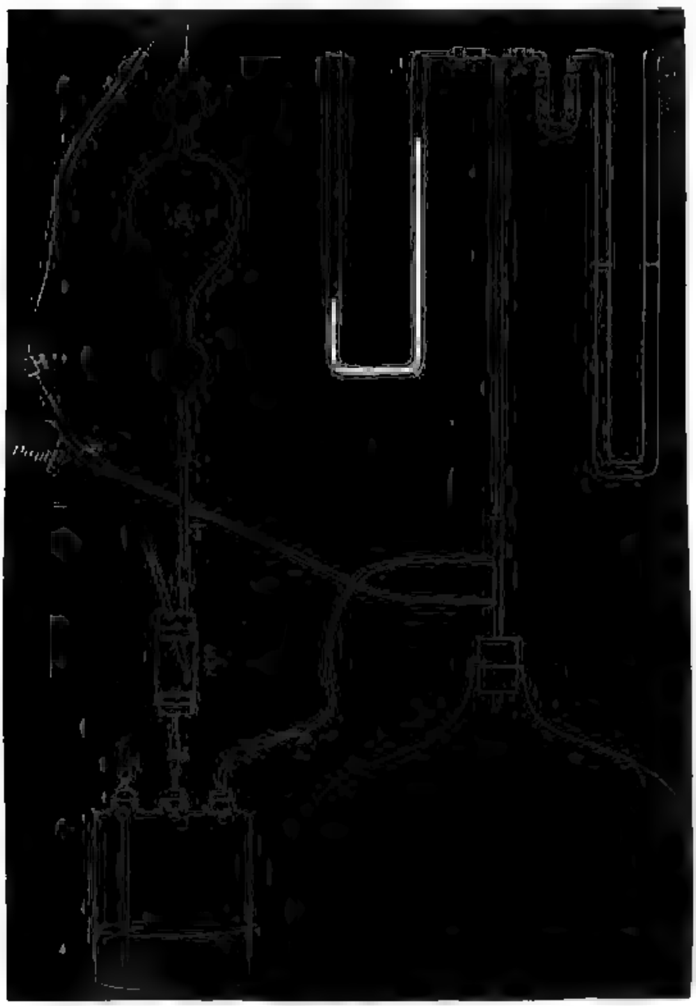
FIG. 1.

I. The water was allowed to run out of a bulb through the transpiration tube under its own pressure.

As the result of four series of observations on three different tubes, it was found that the times of flow for the glass tubes were the same to within 0·7 per cent. as the times for the silvered tubes, corrections being made for changes of temperature, and for the decrease

in diameter due to the silver layer. Different thicknesses of silver were used. The differences in the times were all within the limits of experimental error.

FIG. 2.



II. In order to determine whether any slipping occurred when the gradient of velocity was pushed near the limits of linear motion, as investigated by Prof. Osborne Reynolds,* the difference of pressure

* 'Phil. Trans.,' 1886.

was increased by placing a partially exhausted space in connexion with the lower end of the tube, the size of the bulb being also increased The pressure was read by a kathetometer on a gauge of sulphuric acid or mercury, and a small correction (always less than 0·2 per cent.) made in order to compare the times of flow under the same pressure. The temperature of the water was observed immediately after it had passed the transpiration tube, by reading a thermometer immersed in the cylinder C, at equal intervals, while the water was running. The smallest tubes which could conveniently be silvered had a diameter of rather less than a millimetre, and in this series of experiments the tubes had diameters of 0·084 and 0·072 cm. The results of four series of experiments showed that in three cases the times for the silver and glass surfaces agreed to less than 0·5 per cent., and in one case, where the silver was rather too thick, and probably a little irregular, the time of flow for the silvered tube was 1 per cent. greater.

Even if the slipping effect was only half that deduced by Helmholtz and Piotrowski for gold, the times of flow should have been for these small tubes from fifteen to twenty times less for silver than for glass. The two series of experiments given above may therefore be considered a quite satisfactory proof of identity.

FIG. 3.

III. Girard's experiments on copper tubes give $\lambda = 0·3984$ mm. The times of flow for a tube of 1·83 mm. in diameter were about three times less than that given by Poiseuille's law. These experiments were repeated with some solid drawn tubes, kindly made for me by Messrs. Elliott, of Selly Oak, Birmingham. The diameters were estimated to about 0·1 per cent., by weighing the tube first empty, and then full of water. By some subsidiary determinations with glass tubes, I showed that this gave the same result as the usual method with mercury. The results in all cases showed agreement with Poiseuille's observations. The times of flow were always a little

greater, obviously owing to irregularities in the tubes, but never less.

The surfaces of the tubes were then modified in various ways: by cleaning with acids and alkalis, by polishing with emery powder, by coating with a film of oil, and by amalgamating with mercury. In no case, however, could any deviation from Poiseuille's laws be detected. Girard gives no account of the method he employed for determining the diameters, and this may explain his results; any constant error would of course be more important in smaller tubes than in larger ones, and produce the same result as a slipping coefficient. The tubes I used were some of them smaller than those of Girard, and any slipping would have produced an even greater effect.

IV. At the beginning of Helmholtz and Piotrowski's paper, the latter claims to have shown by experiments on a glass flask, plain and silvered, that the friction exerted on it when oscillating by the contained water depended on the nature of the surface. These experiments were repeated, care being taken to make corrections for temperature, and to prevent alterations in the bifilar suspension, which were very apt to occur. Both of these precautions were neglected by Piotrowski, who only took two observations of the logarithmic decrement and the time of swing for each state of the flask, but deduced a 4 per cent. difference in the frictions.

The results of my observations are

Silvered surface, time of swing ..	8·806 sec.	log. dec.	0·142335
Unsilvered surface „ ..	8·779 „	„	0·142217

By the theoretical part of Helmholtz's paper, this makes the ratio of the friction on glass to the friction on silver

$$1{\cdot}0022 : 1.$$

The change, if any, is less than 0·3 per cent., and the ratio is unity within the limits of experimental error.

The figures given above are the means of twelve observations for the silver, and of twenty-three observations for the glass, some being taken before, and some after, those for the silver.

V. A modification of Piotrowski's experiment was then tried. Instead of filling the oscillating flask with water, it was filled with sand, and oscillated as a rigid body in a beaker of water. The ordinary investigation for such cases was then applicable, and it is easily shown that if k and k' are the frictions, λ and λ' the log. decrements, and T and T′ the times of swing for the two cases,

$$\frac{k}{k'} = \frac{\lambda T}{\lambda' T'}.$$

The outside of the bulb was silvered, observations taken, and the silver then dissolved without touching the suspension, and observations again taken.

Silvered surface ..	9·898 sec.	log. dec.	0·20718
Glass " ..	9·938 "	"	0·20751

Ratio of frictions 1 : 1·00564.

The change is thus less than 0·6 per cent., and is within the limits of experimental error.

The main part of Helmholtz's paper is taken up with the consideration of experiments on the oscillations of an accurately worked sphere. It is remarkable that he deduces a value for the coefficient of viscosity which is about a quarter greater than that given by Poiseuille. This seems to suggest that a slight change in the application of theory to the results of experiment is needed, which will reduce the coefficient for the viscosity of the liquid, and increase the value for its adhesion to the walls of the vessel to that required for the condition of no slip. The existence of any effect approaching in magnitude that given by Helmholtz would produce, as I have shown, such an enormous change in the time of flow through a silvered tube, that the result of my experiments must be considered quite conclusive. The argument from the differences in friction due to differences in surface, in favour of the contact theory of E.M.F. is now seen to be worthless; and it must be admitted that no slip occurs, at any rate with solids that are wetted by the liquid.

III. "Re-determination of the True Weight of a Cubic Inch of Distilled Water." By H. J. Chaney. Communicated by the President. Received February 4, 1890.

(Abstract.)

Recent investigations as to the value of the metric unit of volume —the cubic decimetre—appear to show, indirectly, that the present weight of a cubic inch of distilled water (252·458 grains, $t = 62°$ F., $b = 30$ in.)—the hitherto accepted unit of volume in this country—is appreciably too high. This weight (252·458 grains) is based on weighings made by Shuckburgh in 1798, and on linear measurements by Kater in 1821; but their results are affected by uncertainty as to thermometric and linear measurements, and as to the condition of the water used. Hence a direct re-determination of the unit of volume in this country appeared now to be desirable.

Methods and Apparatus Employed.

The weight of a given volume of water is best determined by ascertaining the weight of water displaced by a body or gravimeter, whose weights in air and *in vacuo*, and external linear dimensions, may be precisely determined. For the purpose of the present experiments three such gravimeters were used:—

C. A platinised hollow bronze circular cylinder, 9 inches in diameter and height.

Q. A quartz cylinder, 3 inches in diameter and height.

S. A hollow 6-inch brass sphere.

The dimensions of C, Q, and S were measured by two comparators, designed for these measurements, geometric lines being traced on C and S for this purpose; to $\frac{1}{100000}$th part of an inch. The actual rate of expansion of each gravimeter by heat was not separately determined, as the probable errors which arise in ascertaining the rates of expansion of bodies of the particular sizes and forms of C, Q, and S, would be larger than the probable errors which arise in applying the rates of expansion obtained from experiments made by the Fizeau optical method on smaller cubes of similar materials.

Water.—For the rate of the expansion of water the mean corrected observations of Despretz, Kopp, and Pierre, as taken by W. H. Miller (1856), and Foerster (1864), have been followed. If those of Hägen and Mathiessen had been included, the weight of the cubic inch would have been affected by $\pm$ 0·0009 grain. For the normal temperature to which we wish to reduce the cubic inch ($t = 62°$ F.), the maximum density of water to its density at $t = 62°$ F. is—

$$\frac{\text{at } 4° \text{ C.} = 1{\cdot}000000}{t_{62° \text{ F.}} = 0{\cdot}998881}.$$

The water was in each case twice distilled; no chlorine, carbonic acid, lead, or lime, being traced, in any quantity to affect the weighings. No correction for the absorption of air was applied, as the distilled water was so far deprived of air, by boiling, and under an air-pump.

Thermometers.—Six standard thermometers, verified both before and after the experiments were used, viz., Centigrade. 4517, 4518 (Tonnelôt); Fahrenheit 430 (Kew Committee), 12765 (Negretti and Zambra), and 20065 (Hicks); the verifications being based on the two thermometers 4517 and 4518, the values of which had been expressed by Dr. René Benoît and Dr. Pernet in relation to the hydrogen thermometers to $\pm$ 0·001° C., each thermometer being corrected for exterior pressure, and its readings reduced to the horizontal position. The experiments were made as nearly at 62° as

Table II.

I. Nature of experiment.	II. Thermometer used.	III. Number of determinations.	IV. Mean temperature.	V. Extreme divergence of any experiment.	VI. Temperature adopted.*	Previous observers.
B. p. of aniline (760 mm.)— Sample I.	F.	6	184·23	+ ·03° − ·01	184·22	Ramsay, 184·41. Thorpe, 183·7.
	E.	3	184·19	+ ·02 − ·01		
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B. p. of mercury (760 mm.)	F.	2	357·68	± ·00	357·65	Regnault, 357·25. Ramsay, 358·2.
	E.	4	357·64	+ ·02 − ·01		
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Freezing point of tin	F.	3	232·00	+ ·02 − ·01	232·03	Reimsdyk, 228·5. Kupffer, 230. Person, 232·7. Crichton, 238.
	G.	4	232·02	± ·02		
	E.	2	232·08	± ·00		

Freezing point of bismuth— Sample I	E.	3	269·68	+ ·02 − ·01		
	G.	3	269·69	± ·01		
Sample II (Johnson and Matthey.)	E.	3	269·69	+ ·02 − ·04	269·68	Person, 270·5. Reimsdyk, 268·3.
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Freezing point of cadmium— Sample I (Harrington Bros.)	E.	3	321·51	+ ·01 − ·00		Person, 320·7. Reimsdyk, 320. Van der Wyde, 325.
	G.	2	321·49	+ ·02 − ·01		
Sample II (Johnson and Matthey.)	E.	2	321·70	± ·02	321·67	
	G.	3	321·64	± ·02		
Freezing point of lead— Sample I (Harrington Bros.)	F.	3	328·27	+ ·02 − ·01		Person, 326·2. Kupffer, 334. Quincke, 330.
Sample II (Johnson and Matthey.)	G.	3	328·79	+ ·00 − ·01	328·78	
	E.	3	328·77	± 02		
Freezing point of zinc	G.	3	421·21	± ·01	421·23	Reimsdyk, 420. Wright and Luff, 420. Person, 433·3. (Other observers range from 342 (Daniell) to 450 (Boussingault).
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In no case would the divergence shown in Column V affect the fourth figure in Column VI.

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Full particulars of the individual experiments are given in the tables attached to the paper, and the results are summarised in the accompanying tables.

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The results given bear out the following conclusions:—

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Putting $r = 0{\cdot}05$ and $\lambda = 0{\cdot}23534$, we get

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† 'Mémoires de l'Institut,' 1813—1815.

Thus the flow through a gilt tube of a millimetre in diameter should be twenty times as fast as through a tube where there is no slip. Poiseuille showed that for a glass tube $\lambda = 0$, and it had been generally supposed that this also held for other substances wetted by water.

Such a large effect as the above shows that the existence of the coefficient would be much better investigated by transpiration experiments than by oscillating spheres, and an investigation has been carried out on these lines.

In order to prevent absolute determinations, the time of flow of a known volume of water through a glass tube was observed, the interior of the tube silvered, and another observation taken with the same pressure and the same volume of water.

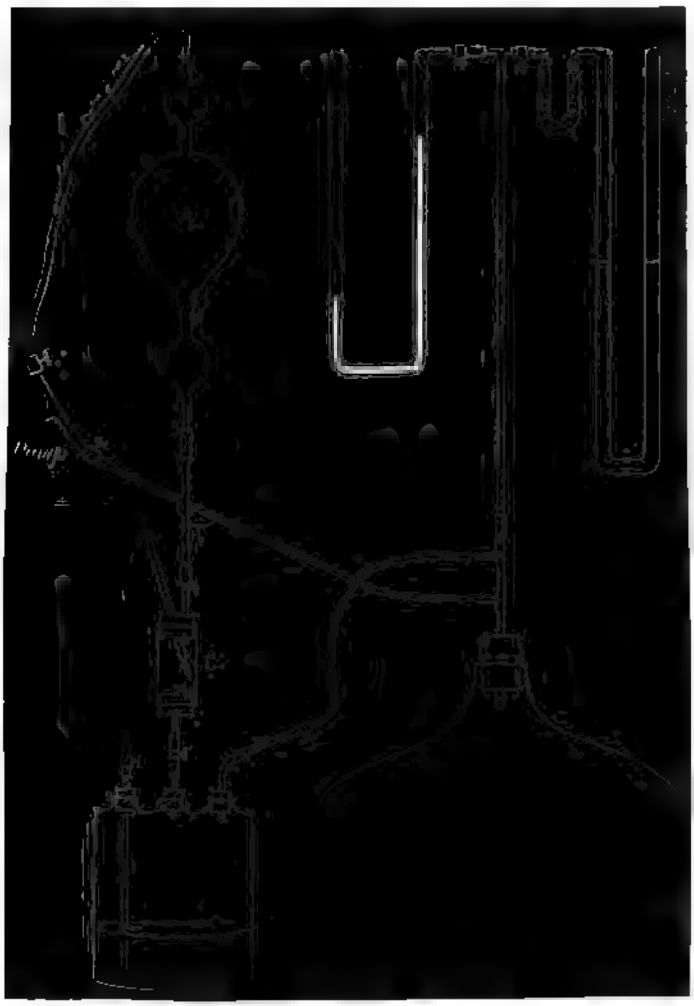
FIG. 1.

I. The water was allowed to run out of a bulb through the transpiration tube under its own pressure.

As the result of four series of observations on three different tubes, it was found that the times of flow for the glass tubes were the same to within 0·7 per cent. as the times for the silvered tubes, corrections being made for changes of temperature, and for the decrease

in diameter due to the silver layer. Different thicknesses of silver were used. The differences in the times were all within the limits of experimental error.

Fig. 2.



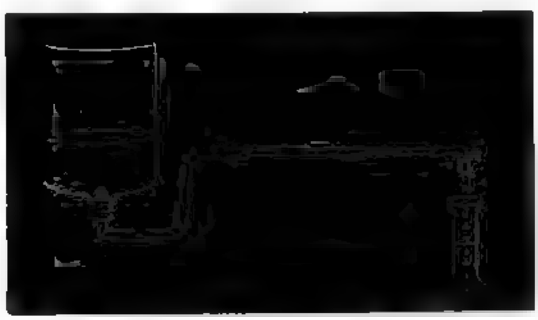
II. In order to determine whether any slipping occurred when the gradient of velocity was pushed near the limits of linear motion, as investigated by Prof. Osborne Reynolds,* the difference of pressure

* 'Phil. Trans.,' 1883.

was increased by placing a partially exhausted space in connexion with the lower end of the tube, the size of the bulb being also increased. The pressure was read by a kathetometer on a gauge of sulphuric acid or mercury, and a small correction (always less than 0·2 per cent.) made in order to compare the times of flow under the same pressure. The temperature of the water was observed immediately after it had passed the transpiration tube, by reading a thermometer immersed in the cylinder C, at equal intervals, while the water was running. The smallest tubes which could conveniently be silvered had a diameter of rather less than a millimetre, and in this series of experiments the tubes had diameters of 0·084 and 0·072 cm. The results of four series of experiments showed that in three cases the times for the silver and glass surfaces agreed to less than 0·5 per cent., and in one case, where the silver was rather too thick, and probably a little irregular, the time of flow for the silvered tube was 1 per cent. greater.

Even if the slipping effect was only half that deduced by Helmholtz and Piotrowski for gold, the times of flow should have been for these small tubes from fifteen to twenty times less for silver than for glass. The two series of experiments given above may therefore be considered a quite satisfactory proof of identity.

FIG. 3.



III. Girard's experiments on copper tubes give $\lambda = 0{\cdot}3984$ mm. The times of flow for a tube of 1·83 mm. in diameter were about three times less than that given by Poiseuille's law. These experiments were repeated with some solid drawn tubes, kindly made for me by Messrs. Elliott, of Selly Oak, Birmingham. The diameters were estimated to about 0·1 per cent., by weighing the tube first empty, and then full of water. By some subsidiary determinations with glass tubes, I showed that this gave the same result as the usual method with mercury. The results in all cases showed agreement with Poiseuille's observations. The times of flow were always a little

greater, obviously owing to irregularities in the tubes, but never less.

The surfaces of the tubes were then modified in various ways: by cleaning with acids and alkalis, by polishing with emery powder, by coating with a film of oil, and by amalgamating with mercury. In no case, however, could any deviation from Poiseuille's laws be detected. Girard gives no account of the method he employed for determining the diameters, and this may explain his results; any constant error would of course be more important in smaller tubes than in larger ones, and produce the same result as a slipping coefficient. The tubes I used were some of them smaller than those of Girard, and any slipping would have produced an even greater effect.

IV. At the beginning of Helmholtz and Piotrowski's paper, the latter claims to have shown by experiments on a glass flask, plain and silvered, that the friction exerted on it when oscillating by the contained water depended on the nature of the surface. These experiments were repeated, care being taken to make corrections for temperature, and to prevent alterations in the bifilar suspension, which were very apt to occur. Both of these precautions were neglected by Piotrowski, who only took two observations of the logarithmic decrement and the time of swing for each state of the flask, but deduced a 4 per cent. difference in the frictions.

The results of my observations are

Silvered surface, time of swing ..	8·806 sec.	log. dec.	0·142335
Unsilvered surface „ ..	8·779 „	„	0·142217

By the theoretical part of Helmholtz's paper, this makes the ratio of the friction on glass to the friction on silver

$$1{\cdot}0022 : 1.$$

The change, if any, is less than 0·3 per cent., and the ratio is unity within the limits of experimental error.

The figures given above are the means of twelve observations for the silver, and of twenty-three observations for the glass, some being taken before, and some after, those for the silver.

V. A modification of Piotrowski's experiment was then tried. Instead of filling the oscillating flask with water, it was filled with sand, and oscillated as a rigid body in a beaker of water. The ordinary investigation for such cases was then applicable, and it is easily shown that if k and k' are the frictions, λ and λ' the log. decrements, and T and T′ the times of swing for the two cases,

$$\frac{k}{k'} = \frac{\lambda T}{\lambda' T'}.$$

The outside of the bulb was silvered, observations taken, and the silver then dissolved without touching the suspension, and observations again taken.

Silvered surface ..	9·898 sec.	log. dec.	0·20718
Glass „ ..	9·938 „	„	0·20751

Ratio of frictions 1 : 1·00564.

The change is thus less than 0·6 per cent., and is within the limits of experimental error.

The main part of Helmholtz's paper is taken up with the consideration of experiments on the oscillations of an accurately worked sphere. It is remarkable that he deduces a value for the coefficient of viscosity which is about a quarter greater than that given by Poiseuille. This seems to suggest that a slight change in the application of theory to the results of experiment is needed, which will reduce the coefficient for the viscosity of the liquid, and increase the value for its adhesion to the walls of the vessel to that required for the condition of no slip. The existence of any effect approaching in magnitude that given by Helmholtz would produce, as I have shown, such an enormous change in the time of flow through a silvered tube, that the result of my experiments must be considered quite conclusive. The argument from the differences in friction due to differences in surface, in favour of the contact theory of E.M.F. is now seen to be worthless; and it must be admitted that no slip occurs, at any rate with solids that are wetted by the liquid.

III. "Re-determination of the True Weight of a Cubic Inch of Distilled Water." By H. J. Chaney. Communicated by the President. Received February 4, 1890.

(Abstract.)

Recent investigations as to the value of the metric unit of volume —the cubic decimetre—appear to show, indirectly, that the present weight of a cubic inch of distilled water (252·458 grains, $t = 62°$ F., $b = 30$ in.)—the hitherto accepted unit of volume in this country—is appreciably too high. This weight (252·458 grains) is based on weighings made by Shuckburgh in 1798, and on linear measurements by Kater in 1821; but their results are affected by uncertainty as to thermometric and linear measurements, and as to the condition of the water used. Hence a direct re-determination of the unit of volume in this country appeared now to be desirable.

Methods and Apparatus Employed.

The weight of a given volume of water is best determined by ascertaining the weight of water displaced by a body or gravimeter, whose weights in air and *in vacuo*, and external linear dimensions, may be precisely determined. For the purpose of the present experiments three such gravimeters were used:—

C. A platinised hollow bronze circular cylinder, 9 inches in diameter and height.

Q. A quartz cylinder, 3 inches in diameter and height.

S. A hollow 6-inch brass sphere.

The dimensions of C, Q, and S were measured by two comparators, designed for these measurements, geometric lines being traced on C and S for this purpose; to $\frac{1}{100000}$th part of an inch. The actual rate of expansion of each gravimeter by heat was not separately determined, as the probable errors which arise in ascertaining the rates of expansion of bodies of the particular sizes and forms of C, Q, and S, would be larger than the probable errors which arise in applying the rates of expansion obtained from experiments made by the Fizeau optical method on smaller cubes of similar materials.

Water.—For the rate of the expansion of water the mean corrected observations of Despretz, Kopp, and Pierre, as taken by W. H. Miller (1856), and Foerster (1864), have been followed. If those of Hägen and Mathiessen had been included, the weight of the cubic inch would have been affected by $\pm$ 0·0009 grain. For the normal temperature to which we wish to reduce the cubic inch ($t = 62°$ F.), the maximum density of water to its density at $t = 62°$ F. is—

$$\frac{\text{at } 4° \text{ C.} = 1{\cdot}000000}{t_{62° \text{ F.}} = 0{\cdot}998881}.$$

The water was in each case twice distilled; no chlorine, carbonic acid, lead, or lime, being traced, in any quantity to affect the weighings. No correction for the absorption of air was applied, as the distilled water was so far deprived of air, by boiling, and under an air-pump.

Thermometers.—Six standard thermometers, verified both before and after the experiments were used, viz., Centigrade. 4517, 4518 (Tonnelôt); Fahrenheit 430 (Kew Committee), 12765 (Negretti and Zambra), and 20065 (Hicks); the verifications being based on the two thermometers 4517 and 4518, the values of which had been expressed by Dr. René Benoît and Dr. Pernet in relation to the hydrogen thermometers to $\pm$ 0·001° C., each thermometer being corrected for exterior pressure, and its readings reduced to the horizontal position. The experiments were made as nearly at 62° as

might be; an uncertainty of 0·2° F. making a difference in the weight of the cubic inch of 0·003 grain.

Weighings.—The weighings were made in three sensitive balances, by Borda's method; the gravimeter being suspended in water to a fixed depth, by a platinum wire. The largest errors in such weighings are those likely to arise from minute bubbles of air carried down by the body which is suspended in water; and as it is impracticable to keep the gravimeter in boiling water, such bubbles must be looked for, and the gravimeter repeatedly re-immersed.

The normal air adopted in these weighings is that at $t = 62°$ F., $b = 30$ inches, containing four volumes of carbonic-anhydride in every 10,000 volumes of air; and also containing two-thirds of the amount of aqueous vapour contained in saturated air; weighed at Westminster, latitude 51° 29′ 53″, at 16 feet above sea-level (g Westminster $= g_{45°}$—1·00057704). A cubic inch of such air weighs 0·3077 grain.

Results of the present Experiments.

C. Mean height	9·002020 inches.
,, diameter	9·004148 ,,
V_c	572·803651 cubic inches.
Weight in air	183676·066 grains.
,, *in vacuo* ...	183797·198 ,,
ΔC...............	1·27049
S. Mean diameter.......	5·992439 inches.
V_s	112·6694096 cubic inches.
Weight in air	28410·307 grains.
,, *in vacuo* ...	28440·779 ,,
Q. Mean diameter	3·083991 inches.
,, height	3·018485 ,,
Weight in air	15426·95495 grains.
,, *in vacuo*...	15429·55515 ,,
ΔQ	2·265425
V_Q	23·04014 cubic inches.

In normal air a cubic inch of distilled water, freed from air, at the temperature of 62° F., was found to weigh—

C..........	252·267 grains.
S..........	252·301 ,,
Q..........	252·261 ,,

By the experiments with the sphere, apparently greater accuracy was obtained than with the cylinders, and in calculating the weight of the cubic inch, a higher value has been assigned to S; or

One cubic inch of water (as above) = grains 252·286±0·002,

of which grains the imperial pound ($t = 62°$, $b = 30$ inches) contains 7,000.

IV. "On Wind Pressure upon an Inclined Surface." By W. H. Dines, B.A. Communicated by the Meteorological Council. Received June 12, 1890.

In accordance with a plan suggested in a memorandum drawn up by Professor Darwin, I have made the following experiments upon this subject, using for the purpose the large whirling machine of 56 feet diameter erected at Hersham.

The apparatus was made by Mr. Munro, and the general arrangement is shown in figs. 1, 2, and 3.

Fig. 1 gives a view as seen from the point towards which the pressure plate P is moving; fig. 2 as seen from the centre of the whirling machine; and fig. 3 as seen from a point vertically above it.

Fig. 1.

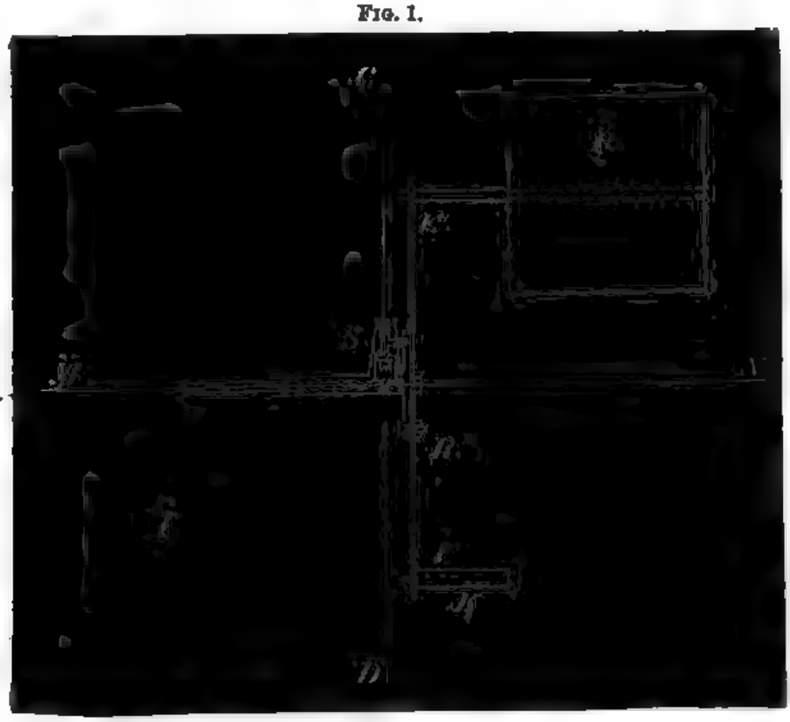


Fig. 2.

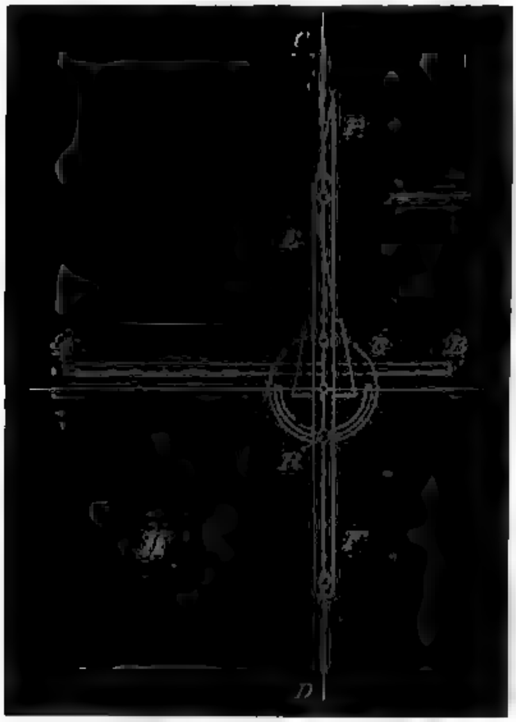
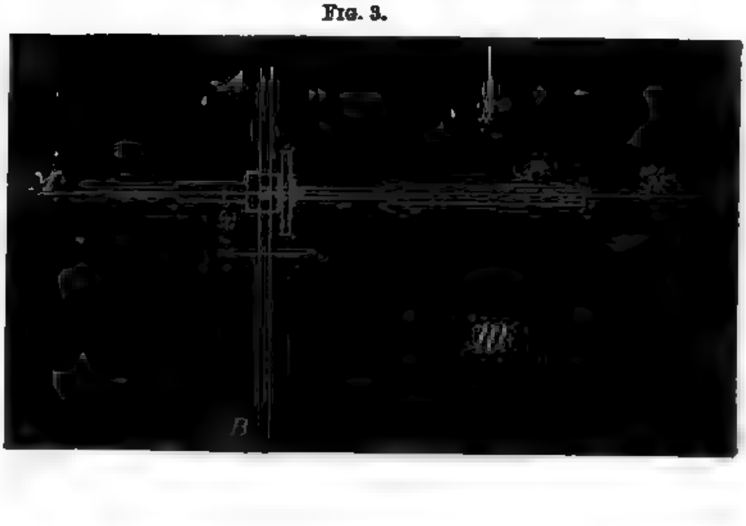


Fig. 3.



The moment due to the wind pressure is balanced by the moment of the centrifugal force due to the circular motion acting upon the bar AB. The axis MN, about which the pressure plate can turn, is coincident with the long arm of the whirling machine, and the axis of rotation CD of the bar AB is vertical, that is, parallel to the axis of the whirler, the bar itself being horizontal, and at right angles to the long arm. The moment due to the centrifugal force could be varied at pleasure by sliding the bar AB longitudinally through a slot, S, and thus altering the distance of its centre from its axis of rotation. The bar weighs 2 lbs., and is graduated in decimals of a foot; thus, if its centre of mass be placed at a distance x from its axis of rotation, the moment is $\frac{2v^2}{gr}x$ (ft. and lbs.).

This arrangement renders any determination of the velocity unnecessary during the experiments, since the wind pressure also varies as v^2, and therefore, as soon as x is known, the moment due to the wind pressure can be expressed in terms of v.

For reasons subsequently explained, it was found advisable to always work at about the same pace, and forty miles an hour was chosen as most convenient.

The pressure plate P of polished wood was 1 foot square, and, in order that the back might not present any irregular surface to the wind, it was made so that the section should be a very obtuse angled isosceles triangle, the altitude being $1\frac{1}{8}$ inches, and the supporting arm passing through the whole width of the solid wood. It was mounted with its centre 1 foot from its axis of rotation, and was balanced by a counterpoise weight, K, placed on the other side of the axis, the weight also making the arrangement symmetrical with regard to wind pressure.

The lever EF, on which the pressure plate was mounted, was clamped to a circular brass disc, G, with a graduated rim, the disc having its centre on the line MN, and being free to turn in its own plane about that line. The disc communicated its angular motion by a stud to the frame, through which the bar AB could slide, the frame and bar being pivoted, so that they could turn about the vertical axis CD, the motion being thus changed from a vertical to a horizontal plane. Of course, a stud being employed instead of a pair of bevelled wheels, a play of only a few degrees was possible. The lever EF could be clamped to the disc by a bolt and nut, R, in any position, and, since the motion was horizontal and the zero mark of the disc corresponded to a vertical position of the lever, the graduated disc afforded an easy method of giving to the angle of incidence of the air upon the face of the plate any desired value. The plate P, again, could be arranged so that the plane of its surface made any desired angle with the lever EF.

Fig. 4.

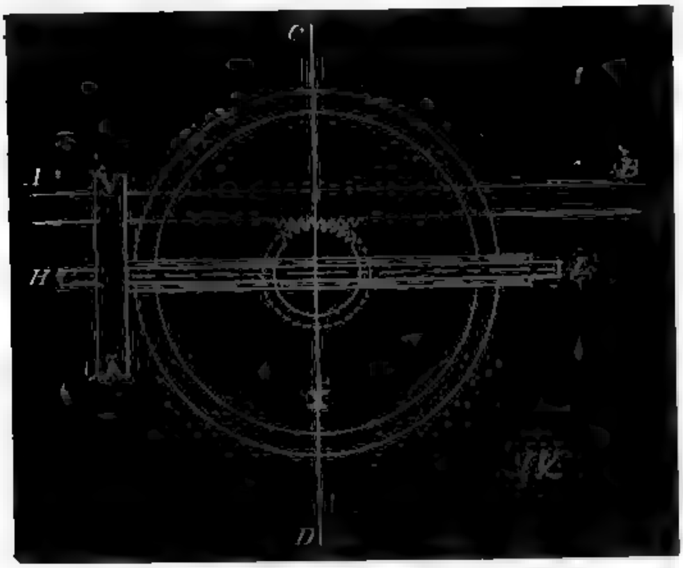
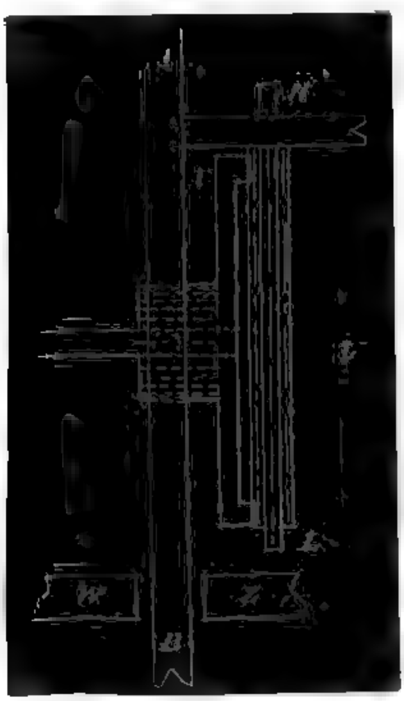


Fig. 5.



It remains to explain how the value of the moment corresponding to any position of the plate was measured. The bar AB was made to take up its position of equilibrium with the plate automatically, and the plan by which this was accomplished is shown in figs. 4 and 5.

The sliding bar AB had a rack cut on it, and the pivoted frame which carried it also carried a crown wheel, X, and pinion, the teeth of the pinion working into the rack cut on the bar. The fixed frame of the apparatus carried a long pinion and grooved pulley, HL. A band from a small windmill placed on the long arm caused this pulley to turn in one direction whenever possible.

The pinion LH was placed just in front of the crown wheel, and so near to it that when it was exactly parallel to the face of the crown wheel the teeth on both sides were engaged, and consequently the arrangement was locked, and no motion could take place, the driving band either slipping on the pulley, or the windmill ceasing to turn.

If, however, the frame carrying the crown wheel moved slightly round its axis CD, so as to bring the bar AB into contact with the stop Z, it is evident the teeth of the wheel on the side near H would become free from the pinion, and, the teeth on the side near L being more deeply engaged, the crown wheel would begin to turn, and would communicate a longitudinal motion to the bar AB, by means of the rack and pinion, causing it to move from B towards A. Contact with the stop W would cause the teeth on the side near H to become engaged, and consequently the wheel would turn in the other direction, and the bar move from A to B. Under these circumstances the bar takes up that position in which the moment due to the centrifugal force is exactly equal to the moment due to the wind pressure which it is required to measure; for that is the only position in which the bar can rest; any departure from the position of equilibrium being immediately followed by a readjustment of the position. Hence, to determine the wind pressure, it was only necessary to clamp the plate in position, to allow the steam engine to give a few turns to the whirling machine, to stop it, and then read off and enter the distance of the centre of mass of the bar from its axis of rotation.

In practice two bars were used, one weighing 2 lbs., which could be placed in any position and clamped by hand; the other weighing $\frac{1}{4}$ lb., which, being worked by the automatic arrangement, made the final adjustment.

Both these bars were graduated in decimals of a foot, and the plan adopted was to enter the distance of the centre of mass of the 2-lb. bar from the axis of rotation first, and then to enter the distance of the small bar, prefixing a + or — sign, according as the two centres were on the same or opposite sides of the axis. Dividing the second entry by 8, since $\frac{1}{4}$ lb. is $\frac{1}{8}$ of 2 lbs., and then adding it algebraically

to the first entry, gave the distance from the axis at which a weight of 2 lbs. would cause equilibrium.

The change of the plane of the couple from the vertical to the horizontal was also accompanied by a change in the length of the arm of the couple in the ratio of 2 : 1, the apparatus being designed thus in order to reduce the weight of the sliding bar; and therefore the value of x, when found as above, had to be multiplied by 4, to give the distance in feet at which the centrifugal force acting upon 1 lb. would balance the pressure upon the plate.

In the event of the small bar having run against its stops at either end, a fresh adjustment of the heavy bar had to be made by hand; but this seldom happened, except in the case of the first determination for a new position.

Position I.

Position II.

Position III.

Position IV.

The four typical positions are shown in the figures, and are referred to in the subsequent tables and remarks as Positions I, II, III, and IV. The following extract, taken from Professor Darwin's memorandum, shows how the normal component of the wind pressure and the position of the centre of pressure may be obtained:—

"It may be supposed that the couple, due to the wind pressure upon all the moving parts except the plate, may be eliminated, so

that the couple necessary to hold the plate in position alone remains to be determined.

"Consider the first and second positions of the plate or vane; since the wind meets the vanes at the same angles in both cases, the couples would be identical, if the centre of pressure were at the middle of the vane. But it is well known that the centre of pressure is nearer the forward edge, and hence the couples are unequal.

"If a be the distance from the centre of the plate to its axis of rotation, and if x be the unknown distance of the centre of pressure from the centre of the plate, and if P be the mean pressure estimated over the whole plate, and L_1 and L_2 the couples corresponding to the two positions, then it is clear that

$$\text{“}L_1 = P(a-x);\ \ L_2 = P(a+x).$$

"from which we easily get

$$\text{“}P = \frac{1}{2a}(L_1+L_2),\ \ x = \frac{L_2-L_1}{L_2+L_1},$$

$$\text{“also } Px = \tfrac{1}{2}(L_2-L_1).$$

"Thus, this pair of experiments gives two of the things to be measured.

"Next consider the 3rd and 4th positions, where the experimental plate is clamped with its plane perpendicular to the arm, and where the inclination to the horizon is complementary to the angle of inclination in the 1st and 2nd positions.

"Suppose that T is the tangential force on the plate, and L_3 L_4 the couples in the two cases.

"Then it is clear that

$$\text{“}L_3 = Ta+Px \text{ and } L_4 = Ta-Px;$$

"from which we get

$$\text{“}T = \frac{1}{2a}(L_3+L_4).$$

"If we avail ourselves of the value of P and x, obtained from the 1st and 2nd experiments, we have

$$\text{“}Ta = L_3-\tfrac{1}{2}(L_2-L_1) = L_4+\tfrac{1}{2}(L_2-L_1).$$

"The 3rd and 4th experiments thus afford a redundant equation, and this may be expected to give a check on the consistency of the results with themselves."—An expectation unfulfilled.

For the purposes of comparison, the value of the moment of the 1 foot square pressure plate, when exposed normally, with its centre

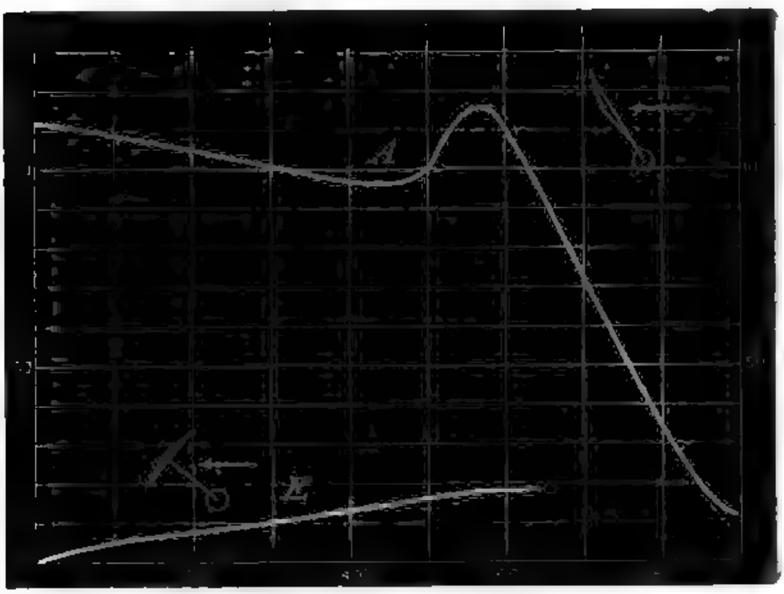
1 foot from its axis of rotation, and when corrected as far as possible for all sources of error, is given as 100, the values of all other moments being expressed relatively to it.

The actual value in terms of the velocity is given subsequently.

Square Pressure Plate.

Curves A and B show the relation between the angle of incidence and the moments for positions I and II, as deduced from the first uncorrected series of experiments, and curves E and F for positions III and IV (figs. 8 and 9). In taking the values of these moments there

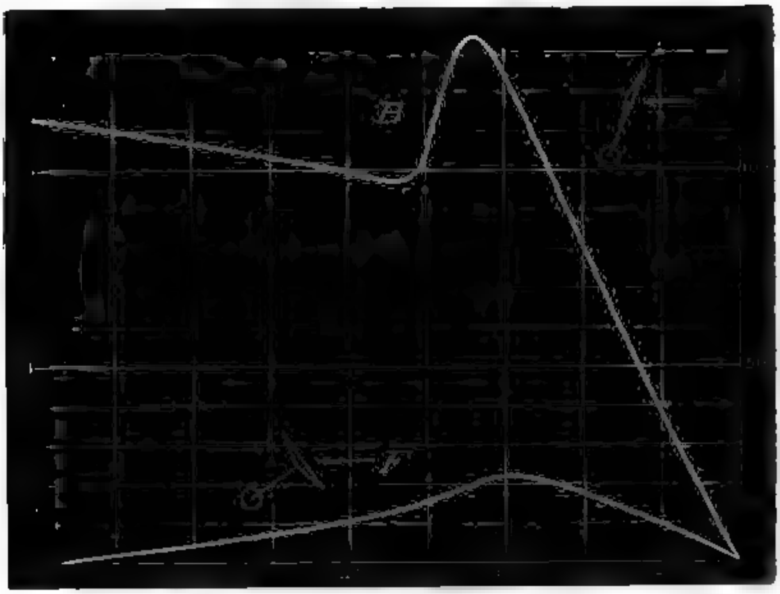
FIG. 8.—Diagram of Moments. Square Plate.



is one point of which we must not lose sight. The long arm of the whirling machine is not perfectly rigid, and gives way under the torsion produced by the wind pressure. Experiments made by hanging weights from the end of a lever showed that a force of 5 lbs. acting 1 foot from the axis caused a deflexion of 2°, and since this is about the moment caused by a velocity of 40 miles an hour, when the plate is exposed normally, the curves have been drawn on the supposition that a moment represented by 100 caused a deflexion of 2°, and that the other moments caused a proportionate deflexion.

It was on this account that it was found advisable to make all the experiments at one uniform velocity.

Fig. 9.—Diagram of Moments. Square Plate.



The actual results which have been obtained are all given in the following tables, in which no notice is taken of the torsional deflexion, although it has been taken into account in drawing the curves.

Position I.		Position II.	
Angle of incidence.	Values of moment.	Angle of incidence.	Values of moment.
10°	97, 105, 115, 107	0°	109, 105, 111, 100, 110, 114, 127, 119, 119 (at top)
15	106	0	107, 100, 114, 106 (at bottom)
20	108, 99	10	106, 108, 108, 108
30	97, 102	15	110, 115
40	97, 96	20	109, 102
45	94	30	103, 100, 97
48	97	40	103, 96
50	94, 91, 99	45	94
51	98	50	96, 95
52	106	52	105, 106
54	109	54	118, 106
55	114, 113, 117	55	124, 106, 107, 126, 123, 126
56	114		
58	112		
60	100, 103, 96, 109		

Position I—*continued.*

Angle of incidence.	Values of moment.
61	102
62	97
64	85
65	79
70	63
75	48, 52, 45
80	38, 39
85	25
90	9, 10, 12

Position II—*continued.*

Angle of incidence.	Values of moment.
56	124, 130
58	144, 151, 144
60	130, 126, 125, 119
61	128, 130
64	125
65	108, 120
67	106
70	73, 89, 85
75	64, 63, 64
80	39, 47
85	25
90	2, −5

Position III.

Angle of incidence.	Values of moment.
5°	0
10	3
15	7, 11
20	9
30	11, 10, 11
45	19
60	22, 15, 22, 15

Position IV.

Angle of incidence.	Values of moment.
5°	0
10	0
15	3, 3
20	4
30	9, 13
40	12
45	15, 14
50	20
52	17
55	21, 23, 24, 22
58	24, 24
60	24, 27, 23, 27, 27
61	24, 23
64	24, 22
65	24
67	24, 23
70	20, 21, 18
75	16
80	7
85	0

These tables give a negative value to the tangential component of the pressure when the angle is 60°, and they also make it appear that the central line of pressure is coincident with the central line of the plate until the angle of incidence exceeds 45°. Neither of

these conclusions seeming probable, it appeared advisable to obtain some independent information upon the point, and for this purpose the natural wind was used.

A light circular disc, of 8 inches diameter, was mounted so that it could turn freely in its own plane, and the lower half was cased in, so that it was completely sheltered. It was then exposed to the wind in various positions, its plane being always vertical, but inclined to the wind direction at different angles. The tangential component of the wind pressure, acting upon one side of the upper half of the disc, would tend to rotate the disc, but it was very seldom that the slightest motion could be obtained, although the friction was so slight that a few grains weight placed on the rim of the disc was sufficient to move it. It seems clear from this that the tangential component is so small that it may be neglected in comparison with the normal, and it has accordingly been considered equal to zero.

To find the position of the central line of pressure, the arrangement

FIG. 6.

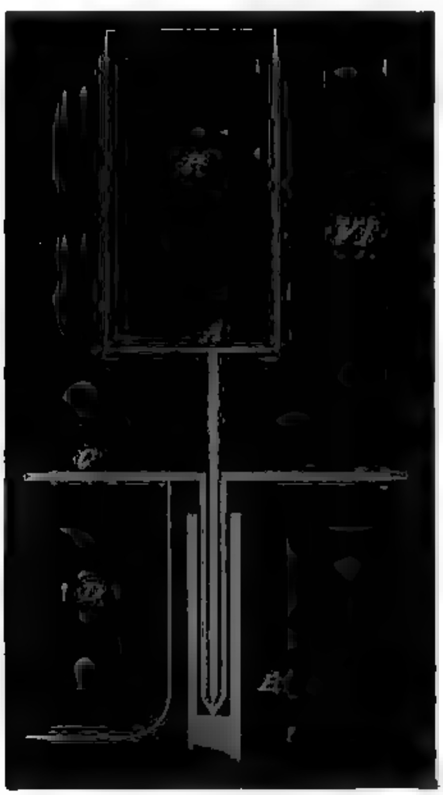


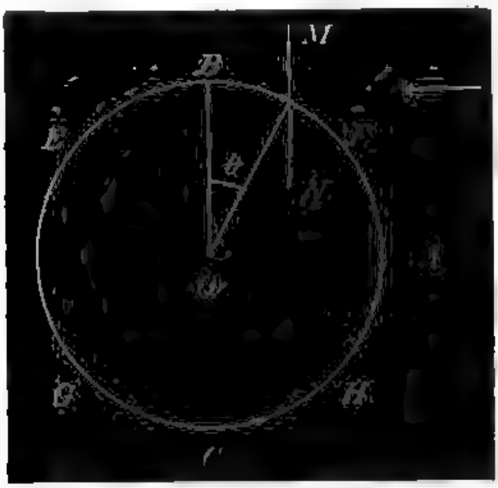
Fig. 7.

shown in figs. 6 and 7 was designed. A pressure plate, P, similar to the experimental plate, was mounted on a steel rod, AB, the line of the rod being on the face of the plate, and parallel to a vertical edge, but not necessarily passing through the centre of the plate. The rod was ground to a sharp point at its lower end and placed in a piece of brass tube with a plug at the bottom, on which the point rested, so that it could turn freely in the tube. On exposing this arrangement to the wind the plate could take up a definite position, the angle between the normal and the wind direction being dependent upon the distance of the centre of the plate from the axis of rotation formed by the steel rod.

It was not possible, however, to measure this angle, because the wind direction never remained steady for a sufficient time. To overcome this difficulty, a flat disc, C, was placed on top of the brass tube, and the tube itself was pivoted in another piece of tube. A definite line on the disc was kept facing the wind by a vane D, and the angle between the normal to the plate and this line was determined, as far as possible, for various positions of the steel rod relatively to the centre of the plate. A vertical section is shown in fig. 6 and a plan in fig. 7. In the following table the angle of incidence is denoted by θ, and the distance between the centre of the plate and centre of pressure by x. Exact relations between x and θ could not be obtained, but the following values are probably within a few degrees of the truth, unless the wind vane below the disc is influenced by the pressure plate. Whether this be so or not I have no means of judging.

$x = 0{\cdot}025$ foot	....	$\theta = 18°$
$x = 0{\cdot}050$,,	....	$\theta = 30$
$x = 0{\cdot}075$,,	....	$\theta = 45$
$x = 0{\cdot}100$,,	....	$\theta = 60$
$x = 0{\cdot}125$,,	....	$\theta = 70$
$x = 0{\cdot}150$,,	....	$\theta = 75$
$x = 0{\cdot}175$,,	....	$\theta = 78$
$x = 0{\cdot}200$,,	....	$\theta = 80$

Reverting to the whirling machine, the inconsistencies shown between the series of results for the different positions appeared to be due to an eddy from the frame of the apparatus, or at least that seemed to me the only feasible explanation. To test this, experiments were made on the machine with the plate MN in the position shown in the annexed diagram, the wind being supposed to come from the



right. If L be the moment about O in this position, the expression L sec θ, which is equal numerically to the force acting upon the plate when exposed normally, ought to be independent of θ, but such was not found to be the case. If a circle be taken with O for centre, and A be taken on the right, so that OA is the direction of motion of the apparatus through the air, and the points F, B, E be taken at 45°, 90°, and 135° respectively from A on the top, and H, C, G be corresponding points on the bottom part of the circle, then the value of L sec θ was found to vary in the following manner:—

At F the value was about 30 per cent. less than at E, the value increasing uniformly from F to E.

At H the value was about 15 per cent. less than at G, increasing uniformly from H to G.

At H and F the values were very nearly equal, F being slightly lower, but the difference was quite within the limits of an accidental error.

These results point clearly to an eddy from the frame of the apparatus, and it was owing to the accidental discovery that the pressure at C was 7 or 8 per cent. less than at B that I was led to try the effect of placing the plate exposed normally in various parts of the circle, as just explained.

Reference to fig. 1 will show how small a surface the frame presents to the wind; actually it is about 14 square inches, or only one-tenth of the surface of the pressure plate, and being as much as 15 inches laterally from the centre of the plate, it certainly seems surprising that it should exert so great an influence.

Observations to find the value of the moment about O for positions I, II, III, and IV were made indiscriminately in the top and bottom segments, before the existence of the eddy was suspected; but subsequent trial showed that the values in the case of oblique exposure were symmetrical about the line AO, excepting in the neighbourhood of the points B and C.

It is clear that the disturbance due to the frame is an important matter, and some attempt must be made to eliminate it.

Since the normal pressure upon the plate was found to increase uniformly from F to E, and also from H to G, it seems to be the least objectionable plan to assume that the moment when the plate is exposed obliquely varies in the same way. This assumption will, at least, bring the position of the central line of pressure, as deduced from positions I and II, more into accordance with the position which it is known to take up.

Also, since the normal pressures at H and F are practically identical, and these are presumably the positions where the eddy from the frame should have the least effect, I think it will be best to take the values of the normal pressure in these positions as the basis with which to compare all other pressures. It will be seen that this is equivalent to taking about 15 per cent. less than the value found at B, or about $7\frac{1}{2}$ per cent. less than the value at C, as the numerical value of the normal component.

Assuming that the tangential component is zero, we have now four ways of determining the normal component for any angle of incidence. They are (1) by combination of the corrected values found for positions I and II; (2) by combination of positions II and IV. These give also the position of the central line of pressure. If we take the position of the central line of pressure obtained from the experi-

ments with the natural wind, these values, combined with positions either II or IV, will give the normal component. It will be seen, on reference to the tables of values found for the various positions, that, in whichever way the curve be constructed, a very curious and sudden rise occurs between the angles of 55° and 60°. Curve C, fig. 10, for the normal component has been constructed by taking the values from positions II and IV, and curve D, fig. 11, by taking the position of the central line of pressure from the natural wind experiments, and then deducing the normal components from the values of the moment in position II. I can form no opinion as to which curve is the more likely to be correct. Some confirmation of the truth of these results is, perhaps, given by the fact that the greatest moment produced by the wind upon a Robinson cup does not occur when the arm is perpendicular to the wind direction, but when there is a considerable inclination, and the fact that a ship can sail at a good pace when its direction makes with the wind direction an angle considerably less than 90° is worth noting.

The position of the central line of pressure, as deduced from the experiments made on the whirling machine, is nearer the front edge of the plate than the position given by direct experiment with the natural wind. It will be seen that in both cases the distance of the central line of pressure from the centre of the plate increases more rapidly with the same change in the angle of incidence, as that angle increases in magnitude; but that the angle at which the acceleration becomes apparent is greater in the natural wind experiments.

The following results, showing the relation between x and θ (see preceding table), may be of interest; they are deduced from positions II and IV:—

$x = 0·025$ foot	....	$\theta = 10°$
$x = 0·050$,,	....	$\theta = 18$
$x = 0·075$,,	....	$\theta = 26$
$x = 0·100$,,	....	$\theta = 33$
$x = 0·125$,,	....	$\theta = 39$
$x = 0·150$,,	....	$\theta = 44$
$x = 0·175$,,	....	$\theta = 49$
$x = 0·200$,,	....	$\theta = 54$
$x = 0·225$,,	....	$\theta = 58$
$x = 0·250$,,	....	$\theta = 63$
$x = 0·275$,,	....	$\theta = 67$
$x = 0·300$,,	....	$\theta = 71$

Rough Surfaces.

The preceding results refer exclusively to a smooth polished plate. The effect of covering the face with sand-paper of medium coarseness,

Fig. 10.—Diagram of Normal Component.
From Positions II and IV.

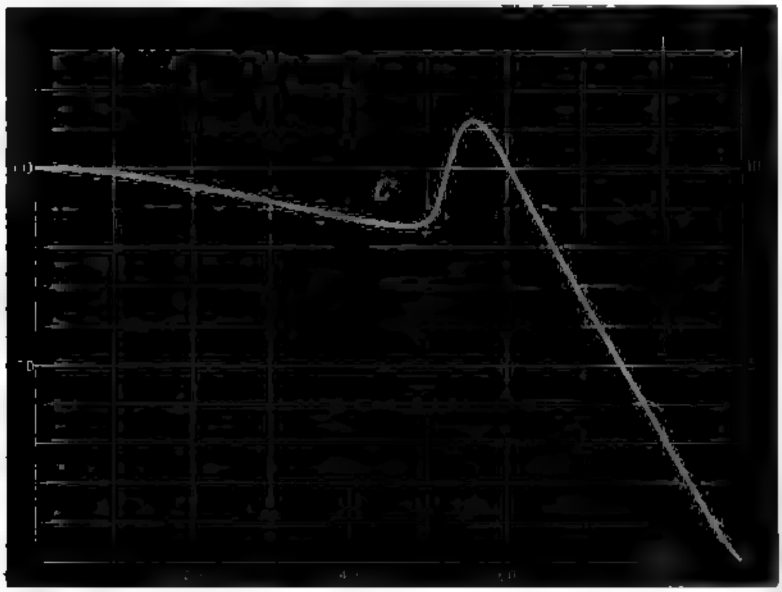
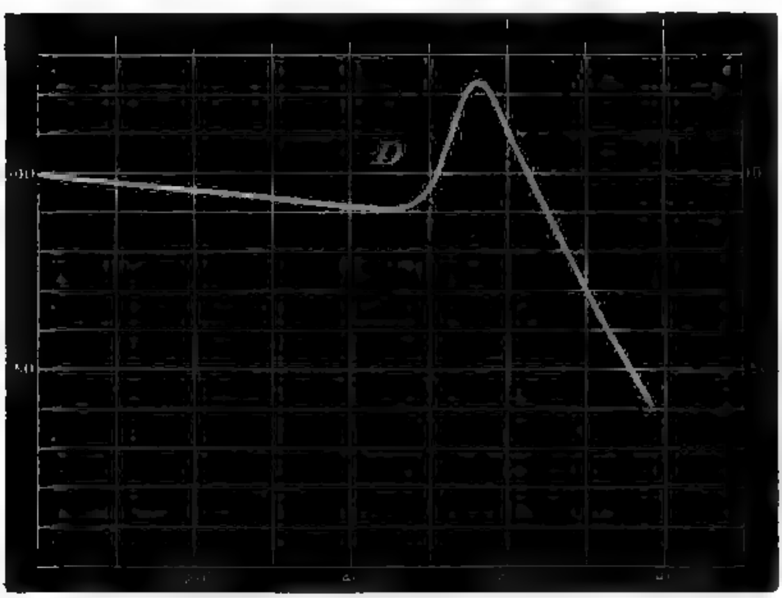


Fig. 11.—Diagram of Normal Component.
From Position II and Natural Wind Experiments.



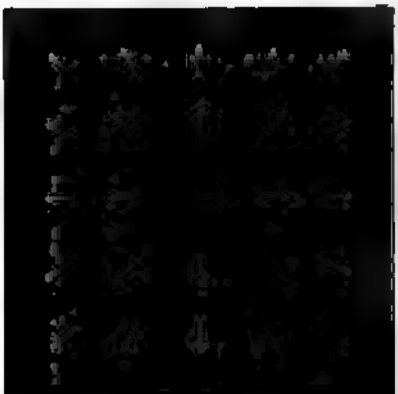
and also with coarse woolly flannel, was tried. So far as I could judge, the sand-paper made no difference in any position, and the flannel made no difference in the normal position. At the angle of maximum moment, the flannel causes a considerable change in the value of the moment. The mean of six experiments made in positions I and II gives a decrease of 21 per cent. in the value of the normal component (not the moment) when the face of the plate is covered with flannel. About the same decrease is caused by thoroughly wetting the face of the plate, probably because a series of ripples are set up, for just damping it has no appreciable effect. Experiments made with the flannel in positions III and IV show that it acts in two ways. It decreases the pressure, and brings the central line of pressure nearer to the centre of the plate; there is also a decided tangential component, but uncertainty about the effect of the eddy in the back positions (I and III) and want of time have prevented my making any attempt to determine the numerical value.

Distribution of the Stream Lines on the Surface of the Plate.

In accordance with suggestions made by Professor Darwin and Mr. Buchan on the occasion of their visit to Hersham on March 20th, an attempt has been made to map out the stream lines near the surface of the pressure plate for various angles of incidence.

For this purpose twenty-five small pins were driven into the face of the board in rows 2 inches apart, and a few short lengths of dark coloured silk were tied to the head of each pin. It was somewhat difficult to see the position taken up by the silk, but the accompanying diagrams give a general idea. Each diagram is the result of two drawings made at different times and compared afterwards, and in cases where the two drawings showed much difference a third trial was made. The pace was about 35 miles an hour, that being very near the limit at which the silk could be seen.

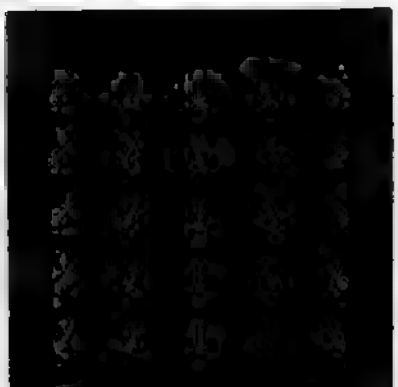
In each diagram the front edge of the plate corresponds to the top, and the left hand to the side nearest to the centre of the whirling machine. The arrows indicate the position taken up by the silk attached to the corresponding pin, and probably show the direction of motion of the air at the point within a few degrees. There is a general tendency of the arrows to incline to the right, which is no doubt due to the centrifugal force. The effect is no doubt indirect, and not due to direct action upon the silk, but to the action upon the comparatively still air, which, being driven to the right, in turn moves the silk. It was evident that in parts where the symmetry of the diagram was destroyed by a turning to the right, the motion of the air was comparatively gentle, this being seen by the inertness of the threads in those positions.



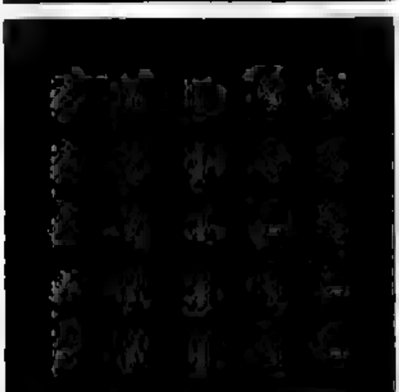
Front 0°.

Front 15

Front 30°.

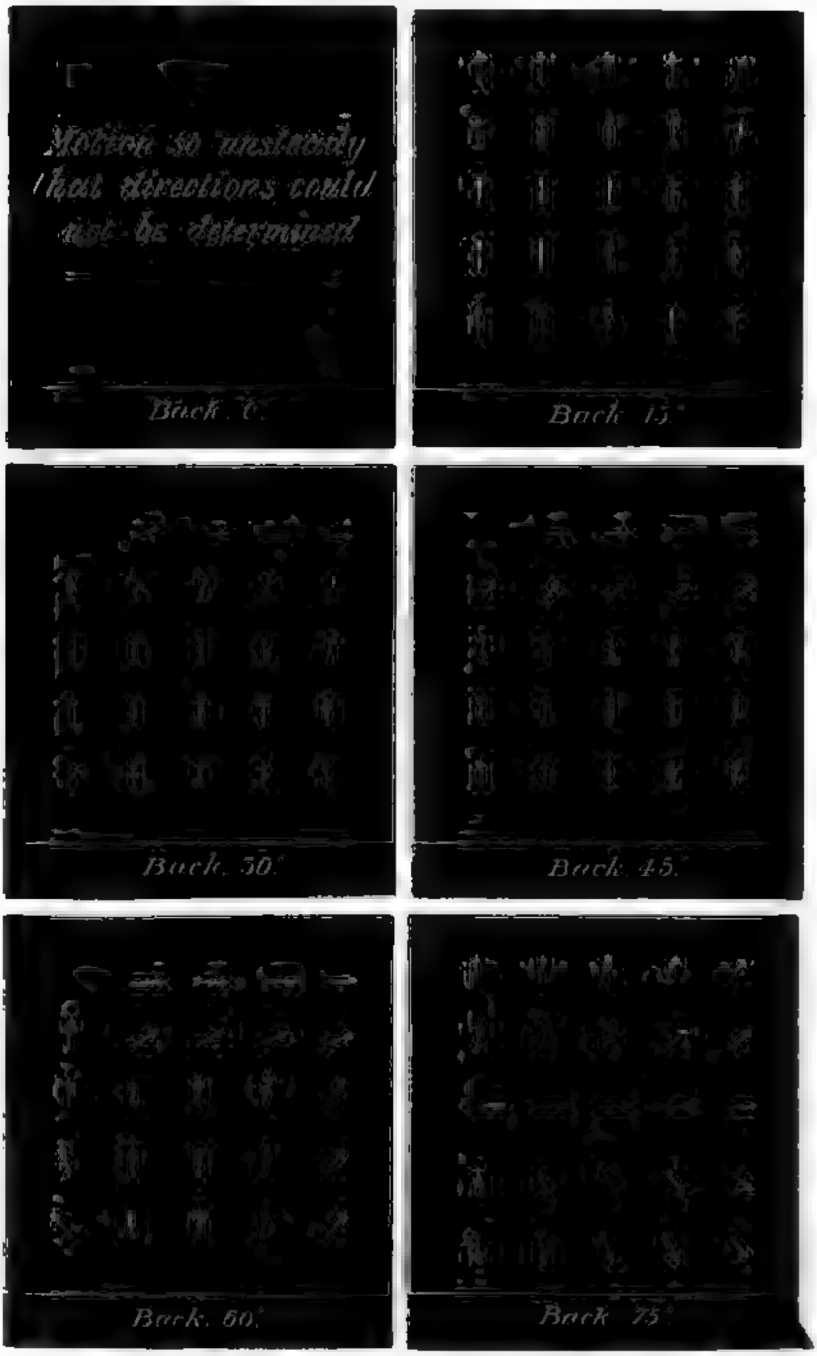


Front 15



Front 60

Front 75


Motion so unsteady
that directions could
not be determined
Back. 0°.
Back 15°.
Back. 30°.
Back. 45°.
Back. 60°.
Back 75°.

No marked change in the distribution of the stream lines occurred on either the back or front of the plate at angles between 45° and 60°, several intermediate positions having been tried.

Until the angle of incidence exceeds 60° there is a region of still air at the back just behind the forward edge of the plate; the width of this seems to increase, but it remains at all events within 2 inches of the edge until the angle of incidence exceeds 60°. It is curious that at 75° the air should be at rest at the centre of the back of the plate, and pass off laterally in both directions. In all cases the motion on the front of the plate was quite steady. At the back, in the case of normal exposure, it was too unsteady to be determined; at 15° it was decidedly unsteady; but in the other positions there was no difficulty in seeing the position of the silk threads.

Effect of Temperature, Pressure, &c.

It will be seen on reference to the tables that very divergent results were sometimes obtained. Obviously experiments made under the same conditions of barometrical pressure, temperature, and wind ought to give the same values. In cases where a small change of inclination causes a considerable change in the moment, a discrepancy in the results is easily explained by supposing a small mistake to have been made in measuring the angle, but in many cases this explanation does not apply. Some natural wind ought to increase the mean pressure, but in so far as I can judge it does not do so to anything like the theoretical amount, that is to say, the pressure is not increased so much as the mean square of the relative velocity. Repeated experiments have shown that it is not possible to get a different result by altering the rate of the whirling machine, all speeds under 70 miles an hour, which is the greatest of which the machine is capable, giving results which show that the pressure varies as the square of the velocity. It must, however, be remembered that at low speeds the forces are too small to be capable of exact measurement.

Barometrical pressure has the result which might be expected, the pressure on the plate varying directly as the height of the mercury. A rise of temperature does not seem to make much difference, but, if anything, it increases the pressure. Experiments have been made through a range of about 40° F., from 28° F. to 68° F. The greater viscosity, I suppose, at the higher temperatures more than compensates for the decrease of density, for certainly, other circumstances being the same, the pressure is not less at 60° F. than at 30° F., and the lowest values ever obtained were in a thick fog with a temperature below the freezing point.

No determinations of the dew point have ever been made in con-

nexion with the experiments, but I think that damp air is conducive to a high, and dry air to a low, relative pressure.

On some days the values will not vary to more than 1 or 2 per cent. throughout; on others, under apparently precisely similar circumstances, variations of 10 or even 15 per cent. will occur within a few minutes. It is perhaps possible that these changes may be due to variations in the viscosity of the air caused by a change in the dew point, or they may be caused by a circular eddy due to the wind coinciding in position with the path of the pressure plate. The latter supposition seems the more probable, but, if so, it ought to appear sometimes in experiments with velocity instruments, and I have tried several air meters many times, and never detected anything approaching to a variation of 10 per cent. It must be remembered, however, that a change of 5 per cent. in the velocity would produce a change of 10 per cent. in the pressure.

These variations give an immense amount of trouble, because it is imperative that an experiment should be repeated many times before the mean value is considered correct.

Actual Pressures.

The actual value of the pressure for any velocity is obtained thus.

In the normal position, the pressure for which has previously been denoted by 100, equilibrium was obtained when the moment due to the centrifugal force was 1·33 (ft. and lbs.). The bar was 27 feet 9½ inches from the centre of the whirling machine, and the centre of the pressure plate 29 feet 1½ inches; hence, since the centre of the pressure plate was 1 foot from its axis of rotation, the pressure P is given in lbs. by the equation

$$P = \frac{1{\cdot}33 \times v^2}{27{\cdot}8 \times 32{\cdot}2},$$

when v is the velocity of the bar in feet per second.

This gives

$$P = \frac{1{\cdot}33 \times 27{\cdot}8V^2}{32{\cdot}2 \times (29{\cdot}1)^2},$$

when V is the velocity of the plate.

Changing to miles per hour, we have

$$P = {\cdot}0029v^2,$$

which gives about 18½ miles per hour as the velocity at which the pressure is 1 lb. per sq. ft.

This is, I believe, a lower value than has been previously given.

The older books on engineering give $P = 0{\cdot}005v^2$, but more recent books give $P = 0{\cdot}003v^2$.

It is a lower value than the one determined at Hersham last year, but I had then no suspicion that the frame of the apparatus would influence the result. It is, however, borne out by the values obtained last year for smaller plates, and the experimental evidence which shows that a decrease of pressure per sq. ft. occurs as the size of the plate is increased.

The following particulars may also be of interest; the method by which they were obtained is described in a paper read before the Royal Meteorological Society in May, 1890. At the centre of the plate when exposed normally the increase of pressure at a rate of 60 miles an hour is equal to 1·82 inches of water, and the decrease at the back, also at the centre close to the plate, is equal to 0·89 inch of water. These values were found with the plate at the point B (see preceding diagram), and, taken in connexion with the fact, discovered I believe by Mr. Curtis, that the pressure in front decreases from the centre outwards, agree fairly well with the value for the pressure obtained in that position.

Long Narrow Vane.

Experiments have also been made with long narrow strips instead of with a square plate. The size chosen was 4 feet long by 3 inches broad, the surface thus being the same as the square plate.

Observations were made at angles of 10° apart, in positions I and II, both with the shorter axis inclined to the wind, and also with the longer axis.

There was a difficulty in mounting these strips so that the supporting arm should not cause any disturbance of the motion of the air over the strip, and still be sufficiently rigid to support the pressure. The thickness of the wood was $\frac{3}{8}$ inch, and the edges were feathered off. It will be seen that when the strip was exposed so that its shorter axis was inclined to the wind the longer axis was necessarily parallel to the long arm of the whirling machine, and its centre 2 feet from the end of the lever. Under these circumstances the velocity would be nearly 4 per cent. greater, and the pressure from 7—8 per cent. greater, than upon the square plate for the same rate of rotation of the whirling machine. This has been taken into account in drawing the curve G.

The support was obtained by a piece of flat iron, $1\frac{1}{4}$ inch by $\frac{3}{16}$, which passed half way along the back of the wood, the end of the iron being bolted to the lever.

The values of the moment for position I were found to be greater than for position II, doubtless on account of the eddy from the

frame, and accordingly the values for position II only were used for drawing the curve.

Rectangle 4 feet by 3 inches.

Shorter Axis inclined to the Wind.

Position II.

Angle of incidence.	Values of moment.
0°	132, 126, 127, 128, 135
10	128, 132
20	135, 126
30	130
40	125, 123
50	114, 120
60	99, 100, 102
70	72
80	37
90	−6

Position I.

Angle of incidence.	Values of moment.
20	133
40	131
50	120, 126, 117
60	120

Longer Axis inclined to the Wind.

Position II.

Angle of incidence.	Values of moment.
0°	126, 129, 125, 126
10	141
20	153
30	167, 156, 159
40	156
50	118
60	84, 93
70	36
75	25, 26
80	11
90	11

Position I.

Angle of incidence.	Values of moment.
10	114, 116
20	93
30	73, 77
40	66
50	54
60	38, 36, 39
70	24
80	24
90	12

The curve G, fig. 12, therefore shows the moment about O, rather than the normal component of the pressure, but, the strip being only 3 inches broad, the departure of the central line of pressure from the centre of the strip cannot cause any very serious difference. The actual pressure upon a surface of this kind is much greater than upon an equal surface when collected in a compact form, such as a square or circle, the difference in this case being more than 20 per cent. This is quite in accordance with previous experiments that have been made on the subject.

The dotted line shows the value of the normal component given by Lord Rayleigh, the curves being made to agree at the beginning and end. The agreement between the theoretical and experimental curves would be more marked if both gave the same quantity, for the moment

FIG. 12.—Long Strip. Diagram of Moments.
Shorter Axis inclined to the Wind.

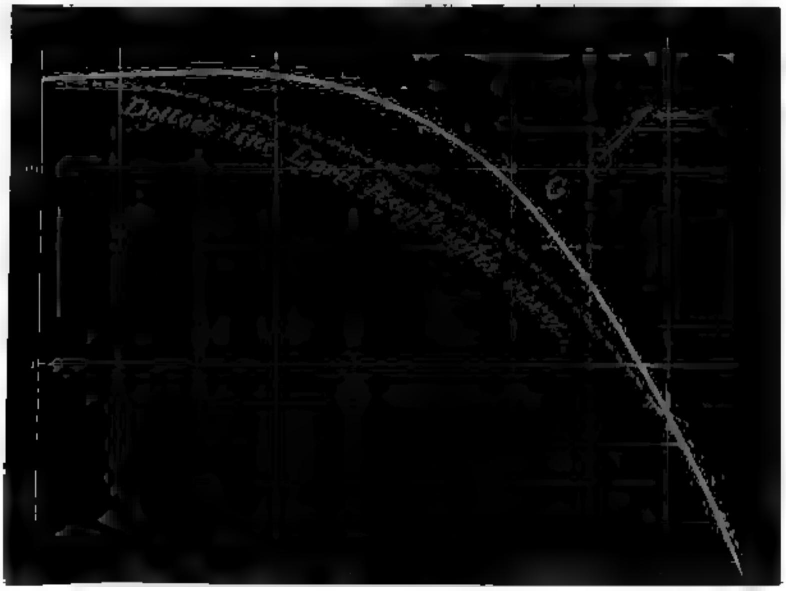
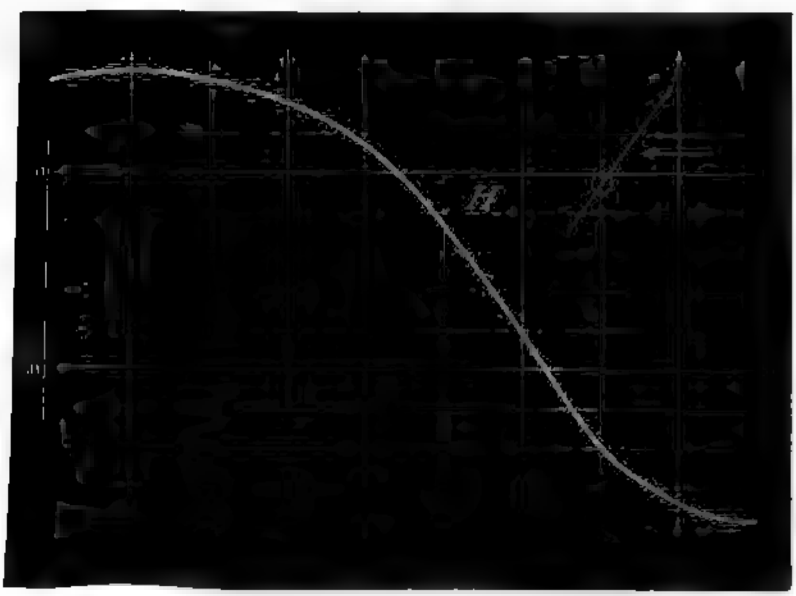


FIG. 13.—Long Strip. Diagram of Normal Component.
Longer Axis inclined to the Wind.



in the intermediate positions is necessarily greater numerically than the normal component.

Curve H, fig. 13, gives the normal component of the pressure upon the same strip of wood with the longer axis inclined to the wind. In this case the wood was mounted 1 foot from the lever (EF, fig. 1) and parallel to it. The curve is drawn from the values of the moments in positions I and II, want of time having prevented my making observations in positions III and IV. The curve shows the normal component, and not the moment, and the distances of the centre of pressure from the centre of the strip are given in the following table, x and θ having the same meanings as before:—

$x = 0{\cdot}10$ foot	....	$\theta = 10^\circ$
$x = 0{\cdot}24$ „	....	$\theta = 20$
$x = 0{\cdot}36$ „	....	$\theta = 30$
$x = 0{\cdot}40$ „	....	$\theta = 40$
$x = 0{\cdot}37$ „	....	$\theta = 50$
$x = 0{\cdot}39$ „	....	$\theta = 60$

It is probable that the flattening of the curve about the angle of 50°, and the corresponding departure from symmetry in the relation between x and θ for that angle, is accidental. The curve is obviously wrong at 90°, where the normal component should be zero, but I have sought in vain for any explanation of this, and can only put it down to the eddy from the frame, which has caused so much trouble in other ways. It is not due to any want of balance, so far as gravity is concerned. Several trials have been made during the course of the experiments to see whether the sliding bar would come to the zero mark when the plate and balance weight were removed from the apparatus, and it has always been found to do so with very fair accuracy. Care has been taken to see that the moving parts were properly balanced for weight, and although the pressure upon the plate, and consequent deflexion, has no doubt slightly altered the balance while the apparatus was moving, the error so caused is very trifling, at least so far as the foot square plate is concerned.

In conclusion, I must say that I am sorry not to have been able to clear up the different inconsistencies better, and not to have been able to draw the curves for the normal component with a greater certainty of their being accurate.

V. "On the Action of Oils on the Motions of Camphor on the Surface of Water." By CHARLES TOMLINSON, F.R.S. Received May 26, 1890.

In Lord Rayleigh's paper on the above subject, read before the Royal Society on the 27th March last, and reported in 'Nature' of the 8th May, it is stated that a film of olive oil, in two or three cases, "was incompetent to stop the camphor motions upon a surface including only a few square inches."

I have often noticed this fact as a consequence of the use of chemically-clean materials. Water, contained in a shallow glass vessel, 4 inches in diameter, on the surface of which camphor fragments were active, was touched with rape oil delivered from the point of a penknife. The fragments continued to rotate on that part of the surface which had not been invaded by the oil film ('Phil. Mag.,' November, 1873). I had previously noticed that a drop of a volatile oil, free from oxidised products, could be spread over the whole surface of the water, without impeding the motion of the camphor fragments, which skated through and cut up the film. In the case of old volatile oils, redistillation was found to be necessary ('Phil. Mag.,' September, 1863). A similar effect was produced by a drop of creosote (or its constituent acids) on a film of a fixed oil that completely covered the surface of the water. The creosote repels the oil film, cuts it up in all directions, moving over the surface with great vigour ('Phil. Mag.,' June, 1867). So also by attention to chemical purity, a raft of mica carrying a bit of camphor will float about briskly on the surface of water night and day during a whole week and upwards ('Phil. Mag.,' December, 1869).

By attending to the chemical purity of the materials the results led to the explanation of many phenomena which had taken refuge under the vague term "molecular change," or "molecular condition," and to the discovery of other phenomena which had some influence in developing theory. I proposed to apply the term *catharised* to bodies thus made chemically clean, from καθαρὸς, "pure" or "clean" ('Journal of the Chemical Society,' April, 1869; also 'Phil. Trans.' for 1870).

I may be allowed to add that, in arriving at the true theory of the camphor motions and their varied kindred phenomena, Professor Van der Mensbrugghe was kind enough to refer to me in his second memoir, as "le physicien qui a le mieux préparé la vraie théorie de ces phénomènes."

VI. "On the Plasticity of an Ice Crystal. (Preliminary Note.)" By JAMES C. McCONNEL, M.A. Communicated by R. T. GLAZEBROOK, F.R.S. Received May 29, 1890.

Two years ago, in the 'Proceedings of the Royal Society,' was published an account of some experiments on the plasticity of ice made by Mr. Kidd and myself. We proved the oft-repeated statement, that glacier ice is not plastic under tension, to be erroneous, and, indeed, that an ordinary bar of ice composed of several crystals will yield continuously either to pressure or tension. But we found that a bar cut out of a single crystal with its length at right angles to the optic axis showed no signs of continuous stretching, even when subjected to half the breaking stress; and other experiments convinced us that an ice crystal will not change its shape under either tension or pressure applied at right angles to its optic axis. These results seemed to render it highly probable that an ice crystal was not in any way plastic, and though, after the winter was over, we wished that we had varied our experiments more, yet we quite expected further experiments only to have corroborated the perfect "brittleness" of a single crystal.

Last winter I resumed the experiments alone. Cutting small bars from uniform crystals, I supported their ends and hung weights halfway between the supports. The result was the discovery of a peculiar kind of plasticity in an ice crystal. The clearest idea of the nature of this plasticity is given by the following analogy:—A crystal behaves as if it were built up of an infinite number of indefinitely thin sheets of paper fastened together with some viscous substance which allows them to slide over each other with considerable difficulty; the sheets are perfectly inextensible and perfectly flexible. Initially, they are plane and perpendicular to the optic axis; and when by the sliding action they become bent, the optic axis at any point is still normal to the sheet at that point. Thus, when a bar with the optic axis transverse to its length is placed so that the axis is horizontal, and the sheets of paper consequently vertical and longitudinal, it refuses to take any plastic bend, however long the weight be applied. If the bar be now turned over, so that the sheets of paper are horizontal, quite a short interval suffices to produce a decided permanent depression of the middle of the bar. In such a case, long narrow bubbles in the ice originally vertical remain vertical, but the optic axis bends with the bar, so that in one half of the bar it is inclined to its position in the other half. The sides of the bubbles were unbroken by steps or "faults," showing that the sliding did not take place at a limited number of surfaces, but was an all-

pervading molecular effect. This conception fully explains our results of two years ago that bars of ice with the axis transverse yield neither to pull nor thrust. If we had tried a bar with the optic axis oblique, it would have stretched readily enough.

The rate of distortion was very irregular, showing a strong tendency to increase with the length of time for which the weight was applied. When extra weight was put on, the rate increased more than in proportion to the weight itself, but less than in proportion to its square. The effect of temperature was generally masked by these others, but there could be no doubt of its existence; the rate at $-2°$ being in one case twice or three times as great as *cœteris paribus* at $-10°$.

Plasticity, due to sliding planes (Gleitflächen), has been shown to exist in rock salt, Iceland spar, &c., by Reusch and others. In rock salt these planes are parallel to the faces of the rhombic dodecahedron, and in general there are several different sets. As long ago as 1867, Reusch suggested their existence in ice as a means of explaining the observed plasticity. I find that the observation that an ice crystal is plastic was made by Hagenbach in 1881, but he did not further investigate the matter.

VII. "Preliminary Note on a New Magnetometer." By W. Stroud, D.Sc., Professor of Physics, Yorkshire College, Leeds. Communicated by Professor A. W. Rücker, F.R.S. Received May 30, 1890.

The determination of the horizontal component of the earth's magnetic field is of great importance, not only for the purpose of magnetic surveys, but also for the determination of the absolute strength of an electrical current, a measurement frequently required, not only for scientific work of various kinds, but also for the calibration of ammeters and voltmeters, and other electrical measuring instruments.

The usual method of measuring this important quantity is that of Gauss, but the method is so long and laborious, and the apparatus requisite for accurate determination so expensive, that the measurement of H is avoided whenever practicable. The writer, having devised an instrument capable of determining H with great expedition and accuracy, ventures to think that a description of the instrument may not be without interest.

Gauss's method consists, as is well known, in finding (1) the deflection produced upon a small magnetic needle by a large magnet of moment M, placed at a known distance from, and in a certain position with regard to, the needle; and (2) the time of vibration of the deflecting magnet when suspended so as to oscillate in a horizontal

plane about its position of equilibrium. The first operation gives the value of M/H, or rather *would* give it if the distance between the poles of the deflectors were known. To measure and allow for this requires a second experiment, when the distance from the needle is altered, and the new deflection read. In this way two simultaneous equations are obtained, from which, by elimination of the distance between the poles, an equation is obtained involving M/H as the only unknown. The second operation gives the product of M and H, or rather *would* give it if the moment of inertia of the magnet about its axis of oscillation were known. As this quantity is not directly determinable, a second equation has to be obtained by increasing the moment of inertia by a known amount, and determining the new time of vibration. In this way two more simultaneous equations are obtained, from which, by elimination of the unknown moment of inertia, the value of MH is obtained in terms of measurable quantities.

Doubtless Gauss's method is excellent when the moment of inertia, as well as the distance between the poles of the deflector, is wanted, but when these quantities are not required (and they never are) a more direct method is very desirable. In a magnetic survey, no doubt, the determination would be shortened by measuring, once for all, the moment of inertia of the deflector, and possibly the distance between the poles.

In place of the laborious dynamical method of measuring MH, various statical methods have been suggested and employed, notably by suspending the deflecting magnet bifilarly and approximately east and west. The instrument to be described consists of a deflecting magnet of peculiar form, suspended bifilarly and approximately east and west; in this case the lower end of the bifilars will be turned through an angle which forms a measure of MH. This magnet at the same time acts at a known distance as deflector to a little magnet, the deflection of which is a measure of M/H. Hereafter the deflecting magnet will be, for brevity, referred to as the "magnet," the little deflected magnet as the "needle."

The magnet consists of a piece of fine pianoforte wire, some 100 cm. long, and 0·06 cm. in diameter, bent into the form of a circle, or approximately so, the two ends being soldered together "end-on," with no overlap. This is magnetised similarly to a Gramme armature, with a north and south pole at opposite ends of a diameter (by placing it with this diameter between the two opposite poles of a weak electromagnet), and so suspended from the bifilar arrangement, that, when the bifilars are vertical, the magnet lies with its plane vertical, and approximately east and west, and with its magnetic axis approximately horizontal. In its position of equilibrium, the couple which the earth exerts upon it is MH $\cos\theta$, where M denotes the

magnetic moment of the magnet, H the horizontal component of the earth's magnetic field, and θ the azimuthal deflection of the magnet. The couple exerted by the weight W attached to the bifilars is $\frac{Wdd'}{4l}\sin\theta$, where d, d' are the distances between the upper and lower ends of the bifilars, and l is their length. Hence we get

$$MH = \frac{Wdd'}{4l}\tan\theta \;\ldots\ldots\ldots\ldots\ldots\ldots (1.).$$

At the centre of the circular magnet there is suspended the little needle, which will be deflected from the magnetic meridian through an angle ϕ. In its position of equilibrium the couple exerted by H is equal to the couple exerted by the magnet. The former couple is $m\text{H}\sin\phi$ where m is the magnetic moment of the needle. Let us imagine for the moment that the whole of the magnetism of the circular magnet is concentrated at two points, one at each end of a horizontal diameter, and let each pole have a strength μ. The intensity of field at the centre is $\frac{\mu}{r^2}$ from each pole, or together $\frac{2\mu}{r^2}$, where r denotes the radius of the circular magnet; so that, neglecting the distance between the poles of the needle, the couple exerted on it by the magnet will be

$$\frac{2\mu m}{r^2}\cos(\phi-\theta) \quad \text{or} \quad \frac{Mm}{r^3}\cos(\phi-\theta);$$

$$\therefore \frac{M}{r^3}\cos(\phi-\theta) = H\sin\phi;$$

$$\therefore \frac{M}{H} = \frac{r^3\sin\phi}{\cos(\phi-\theta)},$$

whence

$$H^2 = \frac{Wdd'}{4lr^3}\,\frac{\tan\theta\cos(\phi-\theta)}{\sin\phi} \;\ldots\ldots\ldots\ldots (2.).$$

If the distances between the bifilars and their length be so adjusted that $\phi \doteqdot \theta$, *i.e.*, that the magnet and the needle turn through approximately the same angle in the same sense, then

$$H^2 = \frac{Wdd'}{4lr^3}\cdot\frac{\tan\theta}{\sin\phi}.$$

Or, if the deflections θ, ϕ be read off in the usual way with telescope or lamp and scale, then, to a certain degree of approximation,

$$H^2 = \frac{Wdd'}{4lr^3}\cdot\frac{\theta}{\phi}.$$

Thus H is determinable in terms of a mass, the value of the acceleration of gravity at the place, certain distances, and the ratio of two deflections.

A possible modification of the method consists in making θ accurately equal to ϕ by varying W, d, or l, or, better, by turning the upper end of the bifilars through a known angle. So far, however, the writer has preferred to adjust the constants of the instrument so that for $H = 0{\cdot}18$, θ shall be nearly equal to ϕ. It will be noticed that if telescope and scale be used there is no necessity to determine the distance of the scale from the magnets except very roughly indeed, as we are requiring the ratio of the tangent and the sine of two not very large angles. Thus the necessity of measuring two angles of deflection instead of one as in Gauss's method is really an advantage, as it obviates the necessity of determining either angle absolutely.*

The special feature of the circular form of deflecting magnet is this—that it is a matter of utter indifference what the distribution of magnetism in it may be, provided it be circular and the little needle be at the centre. This can be readily seen, for if we imagine some north-seeking magnetism situated at an angular altitude χ referred to the centre, the earth's moment on this will vary as $\cos \chi$, but at the same time the intensity of field at the centre resulting therefrom and measured horizontally varies as $\cos \chi$ too, so that not only is the position of the magnetic axis unimportant, but the distribution of magnetism may even be irregular without invalidating equation (2). Moreover, if we are careful in the magnetisation to get the poles in something like the right positions, it is not necessary that the magnet should be absolutely circular; all that is necessary is that the magnet should be circular only in the neighbourhood of the poles. In the above equations, then, r will stand for half the polar diameter of the magnet. Again, with a magnet of moderate dimensions the needle need not be placed rigorously at the centre, since it is in a *minimum* field arising from the action of the two opposite poles on opposite sides. To illustrate this, we may take the case of a magnet, not unduly large, 30 cm. in diameter; then if the little needle, instead of being at the centre, is displaced horizontally 1 cm. on either side, allowing, in fact, a range of 2 cm., the intensity of field at the centre is only increased by 1·2 per cent., so that H will be too small by 0·6 per cent. The needle can easily be arranged within 2 mm. of the centre, and in this case H will only be affected to 1 part in 11,000. It will thus be seen that even for the most accurate work a comparatively small magnet may be used, and the little needle need not be placed rigorously at the centre. All the ad-

* It must be understood that the writer is not recommending that small angles of deflection should be used (see Note appended).

vantages of this form of deflector have not even yet been enumerated. The circular magnet may be made, or, rather, requires to be made, very weak indeed; this arises from the fact that the action of the two poles on the needle is a *summational* instead of being a *differential* one, as in the usual method of performing the deflection experiment. There can be little doubt, too, that it is an advantage rather than the reverse to use only weak magnets in determining the value of H. Lastly, any variation in magnetic moment arising from changes in temperature or other causes does not affect the determination, and, what is a matter of some importance in accurate determinations, there is no correction corresponding to that required in Gauss's method for the varying inductive action of the earth in the different positions which the deflector assumes with reference to the magnetic meridian.

Description of Instrument.

The instrument consists of a rectangular wooden box, ABCD, fig. 1, mounted on levelling screws, and provided with a plate-glass window in one of the large vertical sides, which can be opened for obtaining access to the interior. Attached to the upper side of this box is a second, EF, also provided with a door for adjusting the bifilars G,G in position. The little needle N is suspended at the centre of the large box by a silk fibre some 10 cm. long attached to a brass arm, K, which is screwed into the side of the box opposite to the plate-glass door. Fastened to the needle at right angles to its magnetic axis is a plane mirror (P), 1 cm. in diameter. The needle and attached mirror are prevented from turning completely round by a forked piece of wood, F, which also enables the experimenter to observe when the needle is at the centre of the box.

Soldered to the large circular magnet MMM are two hooks of brass of an indented V-shape, H,H, figs. 1 and 2. These are for suspending the magnet from the brass bar L, which forms the lower end of the bifilar arrangement (figs. 1 and 2). The form of this bar is a knife-edge of brass with a V-notch, Q, near one end, so made with the object of enabling the circular magnet to be unhooked and reversed in position. This eliminates any error arising from the circumstance that the plane of the circular magnet may not be placed accurately magnetic east and west when the bifilars are vertical. A long aluminium wire, W, riveted at each end to the bar, descends in the form of a wide loop, and carries a plane mirror, R, to enable the deflection of the circular magnet to be read off. This mirror is placed just below the mirror P, previously mentioned, so that only one telescope is needed in reading the deflections of both needle and magnet. Soldered to the bar is a strip

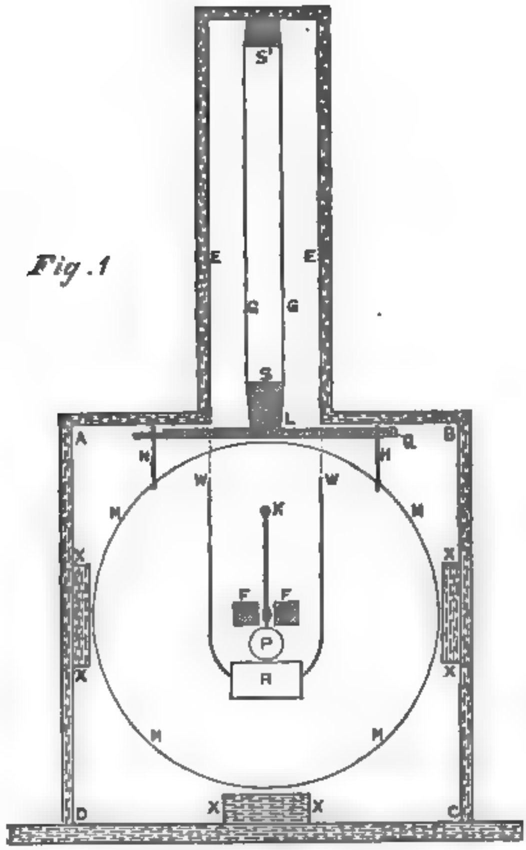


Fig. 1

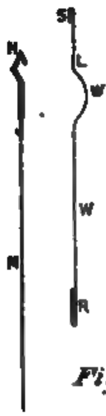


Fig. 2

of thin sheet brass, some 1·3 cm. in width, which is bent over at the top to form a wide hook, S, figs. 1 and 2, under which the bifilars pass. At the top of the instrument is attached a similar hook, S′, from which the bifilars are suspended. The widths of these hooks (which form the distances between the bifilars at the top and bottom) can be read with a micrometer gauge with certainty to 0·001 cm., and by estimation more accurately. The circular magnet, bar, and attached mirror are made as light as they can possibly be made.

Pieces of wood (X, X) are attached to the bottom and two shallow sides of the box in which the circular magnet is suspended in such a way as to leave only about 0·1 cm. clearance for the magnet when it is oscillating before coming to rest. As the needle, too, can only vibrate freely when within a millimetre or so of the centre, it is clear

no observation can be made unless the needle is within at the most 2 mm. of the centre of the magnet.

To read the deflections θ, ϕ, a lamp and scale, or telescope and scale, may be used. A slight difficulty arises with a single telescope and a single scale when setting up the instrument for the first time, owing to the two mirrors not usually making the same or sufficiently nearly the same angles with the vertical. Either two telescopes and one scale, or two scales and one telescope, may be used; but the best plan is to use one telescope and one scale, and to bend the aluminium wire supporting the lower mirror till the latter occupies a suitable position with respect to the vertical.

Corrections will have to be made for (1) the torsion of the silk fibre suspending the needle, (2) the torsion of the silk fibres suspending the magnets, and (3) the couple which the little needle exerts on the magnet. So far as the first two corrections are concerned, they can be allowed for in the ordinary way. For the present instrument the first correction affects H to the extent of one part in a thousand; the second is utterly negligible. It is a matter of interest to determine the magnitude of the couple exerted by the needle on the magnet compared with that exerted by the earth. Now, the earth's couple on the magnet $= \mathrm{MH}\cos\theta$, and on the needle $= m\mathrm{H}\sin\phi$, so that the fractional error in equation (1), made by neglecting this effect, would be $m\sin\phi/\mathrm{M}\cos\theta$, or, practically, $m\phi/\mathrm{M}$, which, for the present instrument, would amount to about $\frac{1}{360}$, since by experiment $\mathrm{M} = 91$ c.g.s. units, and $m = 1{\cdot}5$ c.g.s. units and $\phi = 10°$. This correction is then, by no means, negligible, since it would affect H to the extent of 1 in 700. The error arising from this source could, however, be made very much smaller by diminishing m or, preferably, by increasing M.

The following results have been obtained for the value of H in the physical laboratory of the Yorkshire College, Leeds, which was designed by Professor Rücker, so as to be as free as possible from iron which could not be removed if necessary. The instrument was set up in the middle of the room, the nearest iron being some 4 metres distant, and consisting of a grate, roughly in the same magnetic meridian as the instrument. This will clearly give a higher value for H than if the grate had been removed; but the object of the experiments was to determine H, not for Leeds, but for one place in the physical laboratory.

I. May 17th, 1890. Scale, a metre long, placed roughly 97 cm. from the centre of the instrument.

	cm.
Reading for magnet before reversal ..	10·33
" " " after " ..	52·59
Difference ..	42·26

	cm.
Reading for needle before reversal....	6·21
„ „ „ after „	88·69
Difference....	82·48

Weight suspended from bifilars = 11·37 grams.

Length of bifilars measured by cathetometer = 30·07.

Distance apart of bifilars at top = 0·5092 inch = 1·293 cm.

„ „ „ below = 0·5173 „ = 1·314 „

Diameter of circular magnet = 27·50 cm.

From which data H = 0·1803.

II. May 20, 1890. 4 P.M. Scale 2 metres long, placed 2 metres from instrument. Distance between bifilars below altered and weight changed in consequence.

	cm.
Reading for magnet before reversal ..	153·66
„ „ „ after „ ..	77·34
Difference ..	76·32

	cm.
Reading for needle before reversal....	191·76
„ „ „ after „	25·40
Difference....	166·36

Weight = 11 89 grams.

Distance between bifilars below = 0·5126 inch = 1·302 cm.

Length of bifilars = 27·81 cm. Other constants as before.

H = 0·1805.

III. May 20. 5 P.M. Constants same as in II.

	cm.
Reading for magnet before reversal ..	153·69
„ „ „ after „ ..	77·72
Difference..	75·97

	cm.
Reading for needle before reversal....	192·51
„ „ „ after „	26·67
Difference....	165·84

H = 0·1803.

By comparing experiments II and III, it would seem that the magnetic moment of the magnet had altered, probably owing to handling. This alteration does not, however, in any way affect the result, except in so far as the magnet itself expands with rise of temperature.

Seeing that these results have been obtained with a rough instrument made on the premises, and with very inferior mirrors, the method seems very satisfactory.

For the determination of the strength of a current in absolute measure the writer would suggest placing two coils of wire, each of known geometrical form, the one with its plane in the magnetic meridian on the east side of the instrument, the other similarly on the west side with their axes passing through the centre of the needle. The two coils attached to the instrument would form in fact a Helmholtz standard galvanometer with the addition of the circular magnet with bifilar suspension. Observations of (1) the deflection of the circular magnet on reversal; (2) the deflection of the needle under the action of the circular magnet; (3) the deflection of the needle when the circular magnet is removed altogether and the current traverses the coils will give the value of the current in absolute measure correct, it is believed, to one part in a thousand if the geometrical constant of the coils can be determined to that degree of accuracy.

Now an interesting point arises in connexion with the possible accuracy attainable by this method. The writer believes that with apparatus of the dimensions described there is no difficulty in determining each one of the quantities d, d', l readily to the $\frac{1}{1000}$ part. With a telescope the deflections θ, ϕ can certainly be relied upon to that degree of accuracy, at all events if θ, ϕ are each more than 5°. A little uncertainty arises in connexion with the measurement of r, and this is very important, as $H \propto r^{-\frac{3}{2}}$. Is the pole to be considered at the middle of the wire of the circular magnet, or nearer the surface of the wire, and if so on which side? This question cannot be answered with certainty. Reckoning from the middle of the wire in determining the distance between the poles, the maximum error possible in a wire of 0·06 cm. diameter is 0·03 cm., and this with a radius of 13 cm. gives 1 part in 300 as the extreme error that could be made in H. We may, however, be nearly certain that the pole cannot be more than half the radius of the wire distant from its centre. We may therefore say that about 1 in 500 represents the *possible* error in H arising from this cause. Clearly, however, it is advisable on all accounts to replace the circular wire of the magnet by a flat steel ribbon bent into the form of a circle.

It will be noticed that the effect of variation in temperature in altering the value of the constant of the instrument can be allowed for with great accuracy, as the coefficients of expansion of the

different metals used in the construction of the instrument are known with sufficient accuracy. It is probably better not to include the length of the bifilars in the constant of the instrument, except for rough work, as both variation in temperature and in the hygrometric state of the air will produce sensible alterations in length.

An objection may be taken to the method when very accurate determinations are desired on the ground that a knowledge of the value of the acceleration of gravity at the place of observation is requisite before absolute determinations can be calculated. The writer believes, however, that a quartz fibre suspension for the magnet would be preferable to the bifilar for magnetic survey work. It is perhaps needless to say that provision would be made for clamping magnet and needle during transport. To convert the readings of the instrument into absolute measure, it will be necessary to determine H at as nearly as possible the same place and at the same time by comparing the indications of the instrument with those of a large standard instrument of the bifilar type previously described.

In connexion with the erection of such a standard instrument, the points to be borne in mind are, that all corrections arising from (1) torsion of the silk fibres, (2) uncertainty in the position of the poles of the large magnet, (3) couple exerted by needle on magnet, shall be made as small as possible. To effect this, the bifilars should be longer, the magnet should be made of thin band steel instead of wire, and the diameter of this magnet should be increased in order that r may be measured more accurately, and in order that the magnetic moment of the large magnet may be increased without unduly increasing the deflection of the needle. The writer is at present engaged in erecting such an instrument in the physical laboratory of the Yorkshire College, and hopes to be able to attain with it results approximating in accuracy to 1 part in 10,000.

In conclusion, the present method of determining H is believed to be very much superior to Gauss's in the following respects:—

(1.) The necessity of making a determination of a time of vibration (always a tedious operation) is avoided.

(2.) The determination of a moment of inertia is avoided.

(3.) The determination of the distance between the two poles of the deflector is avoided.

(4.) Variation of magnetic moment of the deflector during the progress of an experiment produces no error; neither does variation in inductive action of the earth produce an error.

(5.) The magnet needs only to be very feebly magnetised, as its action on the needle is due to the *sum* of the actions of the two poles.

(6.) The time occupied in a determination of H is only a few minutes when once the constant of the instrument has been determined.

(7.) The instrument, exclusive of telescope and scale, can be made at a very small cost.

[*Note added June* 17.—In comparing the accuracy of the proposed instrument with the Kew, it is necessary to distinguish between the determination of N in absolute measure, say, for laboratory purposes, such as the measurement of the strength of electrical currents, and determinations where the requirements are the estimation of differences in the value of H at different stations, say, for the purposes of magnetic surveys. For an absolute determination a great deal can be said for an instrument in which the only measurements are certain angles, certain distances, and a certain weight, and which does not require a determination of the influence of the earth's inductive action on the magnet, nor of the variation of the magnetic moment of the magnet with temperature, nor of the position of the poles of the magnet. With reference to magnetic survey work, a comparison may be instituted between the Kew instrument and that under discussion by assuming the constants of each to be known; then in each case angular deflections are being measured, and other things being equal, the accuracy obtainable will be approximately proportional to the magnitude of the angles observed. Now in the Kew instrument, the deflection produced by the deflector in its near position is about 24°, and in its far position about 12°. Something like the difference between these deflections, or 12°, will represent the order of angle to be estimated as accurately as possible. It is not possible in the Kew instrument to materially increase these angles without unduly increasing the influence of the distribution of magnetism in the deflector. In the present instrument there seems to be no reason why deflections of 45° or thereabouts should not be obtained. This reasoning would seem to show that the present instrument could be made considerably more sensitive than the Kew.

It need, perhaps, scarcely be mentioned that the writer is not advocating the use of telescope and scale for measuring the angular deflections in preference to an azimuth circle. The former method (quite unsuitable for measuring large angles) was only adopted in the first instance to roughly test the capabilities of the instrument in the absence of any graduated circle. In a final instrument all the deflections will be referred to a graduated azimuth circle, as in the Kew instrument at present.

A few points of detail may be just mentioned in conclusion:—(1.) To eliminate any error arising in the bifilar suspension from the distance between the centres of the silk fibres being slightly different from the breadth of the metal hook, the writer proposes to control the distance between the upper ends of the bifilars from the *outside* by an aperture in a piece of metal, and to control the distance between

the lower ends from the *inside* by the breadth of the hook. If now the distances between the fibres top and bottom are nearly the same, no sensible error will be made by taking the product of these distances as equal to the product of the breadth of the hook and the width of the metal aperture. (2.) To render the controlling couple produced by the deflected bifilars independent of temperature, it is proposed to select metals with appropriate coefficients of expansion for regulating the dimensions of the bifilars top and bottom, and to alter the length of the silk fibres by an appropriate arrangement, so that a pointer attached to the hook at the lower end shall always come to a fiducial point upon a strip of brass attached to the metal framework which forms the upper suspension. The neatest way of doing this seems to be to cement the plane and silvered side of a short-focus plano-convex lens to the strip of brass, and to arrange its position with reference to the pointer so that the tip of the latter is exactly in the focus of the lens. In this position the tip and its reflected image will appear just in coincidence, and if necessary a lens may be provided in the side of the instrument for observing the relative positions of the pointer and its image and adjusting them to coincidence.]

VIII. "On the course of the Fibres of the Cingulum and the Posterior Parts of the Corpus Callosum and of the Fornix in the Marmoset Monkey." By Charles E. BEEVOR, M.D., F.R.C.P. Communicated by Professor FERRIER, F.R.S. Received June 12, 1890.

(Abstract.)

This paper has for its scope the investigation by the microscope of the course of certain fibre-tracts in the brain which have not hitherto been minutely examined.

After an introduction showing the difficulties of tracing these fibres by dissection and by other means, the *method of investigation* is given. This consisted in cutting serial sections of the brain of the Marmoset Monkey (*Hapale jactans* and *penicillata*) after hardening in bichromate of potash; the sections were stained by Weigert's and also by Pal's hæmatoxylin methods, whereby the fibres are differentiated. In this way, a complete series of sections was made in the sagittal and horizontal planes, and almost a complete series in the frontal direction, and by combining the appearances found in the three planes, a mental picture of the whole could thus be obtained.

In the description of this brain, emphasis is laid on its small size, which renders it very easy of manipulation, while, from its high position in the animal scale, its general arrangement is comparable

with the brain of man. Moreover, the slight amount of convolution on its median surface is a very great advantage in tracing fibres; this is especially the case with the calloso-marginal sulcus, the absence of which enables the fibres of the cingulum to be followed in a way not obtainable in the brains of other apes and of man.

The *cingulum*, or the fibres of the gyrus fornicatus, is described in three parts:—

1. Horizontal, above the corpus callosum;
2. Anterior, in front of this body;
3. Posterior, from behind the corpus callosum to the anterior end of the temporo-sphenoïdal lobe.

The horizontal part consists, not of fibres extending through its whole length, but of internuncial fibres coursing between the gyrus fornicatus and the centrum ovale; the anterior part connects the olfactory nerve with the frontal region; the posterior part contains internuncial fibres between the gyrus hippocampi and the inferior surface of the temporo-sphenoïdal lobe.

The cingulum is not connected with the hippocampal lobule and its contained nucleus amygdalæ, as was considered by Broca.

Reference is made to an operation in the monkey, performed for the author by Professor Horsley, in which the cingulum was divided, producing degeneration in it in a posterior direction.

From the relation of the gyrus fornicatus to sensation found by Professors Horsley and Schäfer, it is suggested that the cingulum joins this gyrus representing sensation with the part of the centrum ovale connected with the so-called motor cortex.

The *calcarine fibres* bounding the calcarine fissure are described as internuncial fibres analogous to the cingulum, and the *superficial fibres* of the gyrus fornicatus are considered to be a separate tract and not part of the cingulum.

The posterior part of the *corpus callosum* is described in three parts:—

1. *The body*, giving off the tapetum to supply the cortex bounding externally the posterior and descending cornua of the lateral ventricle.
2. The *splenium*, ending in the forceps major, sending fibres to the inner part of the occipital lobe below the calcarine fissure.
3. *An intermediate part* between the two former, forming with the tapetum the roof of the posterior cornu, and supplying the cortex of the upper lip of the calcarine fissure.

No connexion between these fibres and those of the internal capsule, as described by Professor Hamilton, can be found.

The *fornix* comprises the body and the posterior crura. The body can be separated, while in the septum lucidum, into (1) a

median and (2) a lateral part. The median part can be traced horizontally backwards into the septum between the body and the splenium of the corpus callosum, but not to join the cingulum as described by Meynert. The lateral fibres descend the lateral ventricle, becoming the tænia hippocampi or fimbria, and end in the cortex of the cornu Ammonis, while the alveus of this body receives fibres from its cortex, and then passes to its under surface to send fibres to the inferior surface of the temporo-sphenoïdal lobe. Besides these parts, there are the transverse fibres connecting the cornua Ammonis of opposite sides.

Particular attention is directed to the different degree of staining by Weigert's method of the corpus callosum, fornix, and tænia semicircularis, of which the last is scarcely coloured, suggesting that it is a degenerated or non-developed structure.

IX. "On the Changes produced in the Circulation and Respiration by Increase of the Intracranial Pressure or Tension." By WALTER SPENCER, M.S., Assistant Surgeon to Westminster Hospital, and VICTOR HORSLEY, B.S., F.R.S. Received June 12, 1890.

(Abstract.)

The authors have made for some time the effect of an increase in intracranial pressure or tension the subject of an experimental inquiry, and they have in this paper recorded the results obtained, in so far as the increase of intracranial pressure affects the circulation and respiration.

They conclude that the increase in intracranial pressure influences the circulation and respiration through the diminution in the physiological activity of the medulla which it causes, and show that the changes produced by the pressure assume a sequence according to the degree to which the activity of the medulla is impaired.

The authors first give an historical *résumé* of the work of previous observers, and then a short introduction on some anatomical and physiological details which relate to the part of the subject under consideration.

The method chiefly employed of increasing intracranial pressure was by inserting a small rubber bag through a trephine hole in the skull, and then distending the bag by means of a column of mercury, which served to show at once the pressure required to distend the bag, and the extent to which the bag was distended.

The capacity of the thin-walled rubber bag, when distended, was at

the same time the measurement of the amount to which the cranial content had been lessened.

The results are divided into two classes:—

I. Those in which the pressure was made on any part of the brain.

II. Those in which it was made by passing the bag into the cavity of the 4th ventricle.

In the former case the heart, the blood pressure, and the respiration were all affected in varying degrees; in the latter case it was found possible to separate the effect upon the heart, the blood pressure, and the respiration respectively.

The following is a summary of the chief results obtained:—

I. *The Heart.*—A considerable increase of the intracranial tension was required to influence the heart; it became slowed and finally arrested. This happened more readily after respiration had ceased, and required a higher pressure to produce it when artificial respiration was employed, whilst division of both vagi nerves abolished any slowing or arrest. The arrest, when produced, continued permanently, unless the pressure was quickly removed, or artificial respiration employed, or the vagi divided. But if the pressure was maintained whilst artificial respiration enabled the heart to start again, then the cardio-inhibitory influence was gradually lost, so that the heart returned from being very slow to its normal rate, or increased beyond the latter until the rate became equal to that seen after division of the vagi. When the vagi were divided at this stage the rate of the heart did not alter. But if the pressure were removed, and a pause made (the vagi being intact), the cardio-inhibitory control was gradually regained, so that the heart could be slowed, or arrested afresh.

The Blood Pressure.—A primary rise, small in the dog, larger in the monkey, was followed by a fall distinct from that produced by the slowing of the heart, and not necessarily accompanying it. When the heart started again the blood pressure rose, finally reaching the level seen after division of the vagi, so that no further rise took place when this was done. But the power of producing a fall of blood pressure was easily lost, for when the intracranial tension was raised for the second time no fall took place, and the blood pressure continued at a normal level or above, even although the heart was greatly slowed. After division of the vagi the blood pressure was raised by increasing the intracranial tension and by artificial respiration, so that it could be maintained at a level between 300 and 400 mm. Hg for considerable periods.

Respiration.—This was likewise impaired and arrested. Its arrest reacted upon the heart and the blood pressure upon it, so that after

the rise of blood pressure respiration occurred, even although a much higher intracranial tension was maintained than had been sufficient to arrest it when the blood pressure was lower.

II. By the direct application of pressure in the upper part of the 4th ventricle a slowing of the heart with a rise of blood pressure was caused, whilst rapid respiration continued, so rapid as even to be nearly three times the rate of the heart in some cases. Pressure below the calamus scriptorius arrested the respiration without directly influencing the heart, whilst in the lower part of the 4th ventricle respiration was impeded or arrested along with a fall in blood pressure, and some slowing of the heart, followed by arrest, after the respiration had ceased.

Numerous observations are recorded which are, in many cases, combinations of the foregoing, and therefore not suitable for condensation in this abstract.

Tracings are furnished illustrating each point advanced.

X. "On the British Earthquakes of 1889." By CHARLES DAVISON, M.A., Mathematical Master at King Edward's High School, Birmingham. Communicated by Professor T. G. BONNEY, D.Sc., F.R.S. Received June 16, 1890.

(Abstract.)

The nature of the evidence on which the accounts are founded is stated, and the method of study described. If the disturbed area be of small dimensions, and if its boundary be approximately circular or slightly elliptical in form, it is assumed that the centre of the area coincides very closely with the epicentrum of the earthquake. During the year 1889 there were at least five earthquakes whose epicentra were situated within the area of the British Islands.

1. *Edinburgh Earthquakes, January* 18.—(*a.*) First shock about 4 h. 10 m. Intensity (according to the Rossi-Forel scale, of which a translation is given) about V. Very little is known about this shock.

(*b.*) Second shock, 6 h. 53 m. Intensity VI. The disturbed area is slightly elliptical in form, being 30 miles long from north to south and $26\frac{1}{2}$ miles broad from east to west; area about 620 square miles. In most places the shock consisted of a single vibration. The characteristic earthquake-sounds were heard in many places, and these places are confined to an area which is neither coextensive nor concentric with the disturbed area. The epicentrum is at a point about 3 miles W. 42° S. of Balerno, and the centre of the sound-area about $2\frac{1}{2}$ miles to the south or south-east of this point. The earthquake was probably

connected with the first of the N.W. and S.E. faults of the Pentlands on the north-west side of the axis; a fault which passes close by the centre of the sound-area, and has a downthrow to the north-west. The inclination of this fault is unknown, but is probably about 75° to the horizon; the depth of the seismic force may therefore be about $8\frac{1}{2}$ miles. It is shown that the earthquake was probably caused by the impulsive friction produced by a slip of the fault referred to; that this slip took place near the middle of the length of the fault; that the slip increased the throw of the fault; that the slip-area was very short, possibly less than a mile in horizontal length, but that it extended from a depth of several miles to within a short distance of the surface.

2. *Lancashire Earthquake, February* 10.—22 h. 36 m. Intensity VI. The disturbed area is nearly circular in form, about 55 miles in diameter, and 2480 square miles in area. The nature of the shock varied with the position of the place of observation. In, or nearly in, a line with the Irwell fault the number of vibrations was generally greater than in places more remote. As in the Edinburgh earthquake, the usual sounds were heard in many places which are confined to a nearly circular area, which is neither coextensive nor concentric with the disturbed area. The duration of the sound was generally greater at places in, or nearly in, a line with the Irwell fault than at places more remote. The epicentrum, which is probably coincident with the common centre of the disturbed area and of the isoseismal line of intensity V, is at a point 2 miles N.N.E. of Bolton, and the centre of the sound-area is about $3\frac{1}{4}$ miles S.S.E. of the epicentrum. The earthquake was probably caused by a slip of the great Irwell fault, which passes close by the centre of the sound-area, having a downthrow to the north-east. If so, the slip must have increased the throw of the fault. The horizontal length of the slip-area was possibly not much more than a mile. The seismic focus is perhaps at a depth of about $3\frac{3}{4}$ miles, but the slip seems to have extended to within a short distance of the surface.

The excentricity of the sound-area in these two cases throws light on the origin of the sound-vibrations. Seismographic records show that near the beginning of an earthquake the period increases with the amplitude, and it is suggested that the sound-vibrations are the very minute vibrations of short period which proceed from the upper and lateral margins of the slip-area. It is pointed out that this theory explains all the known characteristics of earthquake-sounds.

3. *Ben Nevis Earthquake, May* 22.—13 h. 58 m. Intensity about IV. This shock may have been connected with the great fault which crosses Scotland from Inverness in a south-west direction.

4. *Kintyre Earthquake, July* 15.—About 18 h. Intensity V. The disturbed area is roughly elliptical in form, the longer axis being in

a direction about N. 30° E. to S. 30° W.; it is about 25 miles long and 18 miles broad, and about 350 square miles in area. The sound-area appears to be coextensive with the disturbed area, but the observations are too few in number to prove this. The epicentrum is about 3½ miles south-east of Clachan. Dr. Lapworth, in a note to the author, describes briefly the geological structure of the disturbed area, and remarks that its longer axis coincides in direction with the theoretical position of the southern zone of contrary movement in that district.

5. *East Cornwall Earthquake, October* 7.—About 13 h. 45 m. Intensity IV. The disturbed area is elliptical in form, 25 miles long and 20 miles broad, the longer axis running east and west, and about 400 square miles in area. The nature of the shock varied somewhat throughout the disturbed area, but its intensity was very nearly constant. Near the centre of the area the earthquake-sounds were the most prominent feature, but towards the boundary these died out. The sound-area may, however, have been coextensive with the disturbed area, and it is probable that the sound-focus is nearer the surface than the seismic focus. The epicentrum is at a point about 2¾ miles south-west of Altarnon, which is not far from the centre of the great granite boss of eastern Cornwall. The longer axis of the disturbed area is also parallel to the axis of folding of the district.

Doubtful Earthquakes.—Two shocks, supposed to be those of earthquakes, are briefly described, but the evidence is insufficient to prove their seismic origin:—(1) Little Rhondda Valley (S. Wales), June 22, about 22 h. 30 m.; (2) Lyme Regis, July 5, between 23 h. and 23 h. 15 m. The former of these may possibly have been caused by subsidences of the rock over worked-out portions of the coal mines.

In conclusion, the differences between British and Swiss earthquakes are pointed out. The former are rare, and their disturbed areas more or less circular, indicating short fault-slips; the latter are frequent, and their disturbed areas elongated, their axes being parallel to those of the neighbouring mountain ranges, and the fault-slips correspondingly long. They are witnesses respectively of comparatively late and early stages in the process of mountain evolution.

XI. "On the Harmonic Analysis of Tidal Observations of High and Low Water." By G. H. Darwin, F.R.S., Plumian Professor and Fellow of Trinity College, Cambridge. Received June 17, 1890.

§ 1. *Introduction.*

Extensive use of the tide-gauge has only been made in recent years, and by far the largest number of tidal records consist only of observations of high and low water (H. and L.W.). Such observations have usually been reduced by determining the law governing the relationship between the times and heights of H. and L.W. and the positions of the moon and sun. This method is satisfactory so long as the diurnal inequalities are small, but it becomes both complex and unsatisfactory when the diurnal inequality is large. In such cases the harmonic notation for the tide is advantageous, and as, except in the North Atlantic Ocean, the diurnal inequality is generally considerable, a proper method of evaluating the harmonic constants from H. and L.W. observations is desirable.

The essential difference between the method here proposed and that followed by Laplace and his successors is that they introduced astronomical considerations from the first and applied them to each H. and L.W., whereas the positions of the sun and moon will only be required here at a single instant of time. In their method, the time of moon's transit, and hence the interval, was found for each tide; the age of the moon, and the moon's and sun's parallaxes and declinations were also required. An extensive table from the astronomical ephemeris was thus necessary, and there still remained the classification of heights and intervals according to the age of moon, and two parallaxes, and two declinations. The classification could hardly be less laborious, and was probably less mechanical, than the sorting processes employed below. There is probably, therefore, a considerable saving of labour in the present method, and, besides, I conceive that the results are more satisfactory when expressed in the harmonic notation.

My object has been to make the whole process a purely mechanical one, and, although nothing can render the reduction of tidal observations a light piece of work, I believe that it is here presented in a form which is nearly as short as possible.

The analytical difficulties to be encountered in such a task are small, but the arrangement of a heavy mass of arithmetic, so as to involve a minimum of labour and therefore of expense, is by no means easy. How far I have succeeded must be left to the decision of those who will, I hope, use the methods here devised.

When a question of this kind is attacked, the solution cannot be deemed complete unless the investigation is left in such a state that an ordinary trained computer is able to use it as a code of instructions by which to reduce a series of observations, without any knowledge of tidal theory.

An actual numerical example is thus essential, both to test the method and to serve as instructions to a computer. The Appendix contains so much of the reduction of three months of observation at Bombay as will serve as such a code. If the series be longer than three months, or in such cases as the proper treatment of gaps in the series, it is necessary to refer back to the body of the paper for instructions.

I now pass to the theoretical reasons for the rules for reduction.

§ 2. *Notation.*

The notation of the Report to the British Association for 1883, and in use in the Indian tidal work and elsewhere, is here followed.

The earth's angular velocity is denoted by γ; the hourly mean motions of the moon, sun, and lunar perigee by σ, η, ϖ ($\gamma\tilde{\eta}$, $\sigma\epsilon\lambda\acute{\eta}\nu\eta$, $\tilde{\eta}\lambda\iota o\varsigma$); the mean longitudes of moon, sun, and lunar perigee by s, h, p, and the mean solar hour angle by t. The R.A. and longitude in the lunar orbit of the intersection of the equator with the lunar orbit are ν, ξ; and N is the longitude of the moon's node.

The several harmonic tides are denoted by arbitrarily chosen initial letters. Those with which we shall principally have to deal are—

Semi-diurnal.

Name.	Initial.	Speed.	Equilibrium argument
Principal lunar	M_2	$2(\gamma-\sigma)$	$2t+2(h-\nu)-2(s-\xi)$
" solar	S_2	$2(\gamma-\eta)$	$2t$
Luni-solar....	K_2	2γ	$2t+2(h-\nu'')$
Larger elliptic	N	$2\gamma-3\sigma+\varpi$	$2t+2(h-\nu)-2(s-\xi)-(s-p)$
Smaller "	L	$2\gamma-\sigma-\varpi$	$2t+2(h-\nu)-2(s-\xi)+(s-p)+\pi$

Diurnal.

Luni-solar....	K_1	γ	$t+(h-\nu')-\frac{1}{2}\pi$
Lunar	O	$\gamma-2\sigma$	$t+(h-\nu)-2(s-\xi)+\frac{1}{2}\pi$
Solar	P	$\gamma-2\eta$	$t-h+\frac{1}{2}\pi$

The symbol H denotes the mean semi-range of any one of the tides, and κ its retardation of phase behind what it would be according to the equilibrium theory; f denotes a certain factor of augmentation of the lunar and luni-solar tides depending on the value of N.

The particular tide to which H, κ, f refer will in general be indicated by a subscript small letter, the same as the letter constitut-

ing the initial of the tide. Thus, for example, the M_2 tide is expressed by

$$f_m H_m \cos(2t + 2(h-\nu) - 2(s-\xi) - \kappa_m).$$

I have allowed a departure from this notation in the case of the tides K_2 and K_1, where I write H'', κ'', f'' for the first, and H', κ', f' for the second. The angles $2\nu''$ and ν' (which, like ν and ξ, are functions of N) are also involved in the arguments* (or angle under the cosine in the expression for the height of the particular tide) of these two tides.

It is obviously necessary to suppose the reader to have some acquaintance with the harmonic notation, or it would be necessary to repeat the Report on Tides above referred to.

§ 3. *The General Method of Treating H. and L.W. Observations.*

Noon of the day on which the observations begin is to be taken as the epoch, and the mean solar time elapsed since epoch is noted by t. V with the proper subscript letter denotes the increase of argument since epoch; for example, $V_m = 2(\gamma - \sigma)t$.

Then the height of the water h, estimated from mean sea-level, is expressed by a number of terms of the form $A\cos V + B\sin V$, or, in an alternative form, $R\cos(V - \zeta)$.

In order to explain the principle of the method proposed, let us take two typical terms involving V_p and V_q, and let the rates of increase of V_p be p, and of V_q be q.

Then we have

$$h = A_p \cos V_p + B_p \sin V_p + A_q \cos V_q + B_q \sin V_q \quad \ldots\ldots \quad (1).$$

Since at H. or L.W. h is a maximum or a minimum, we must have—

$$0 = A_p \sin V_p - B_p \cos V_p + \frac{q}{p} A_q \sin V_q - \frac{q}{p} B_q \cos V_q \quad \ldots\ldots \quad (2).$$

Let us write

$$\frac{q}{p} = k_q \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3).$$

Then multiply (1) by $\cos V_p$ and (2) by $\sin V_p$, and add; and again multiply (1) by $\sin V_p$ and (2) by $\cos V_p$, and subtract, and we have—

$$\left.\begin{aligned} h\cos V_p = A_p + A_q(\cos V_p \cos V_q + k_q \sin V_p \sin V_q) \\ + B_q(\cos V_p \sin V_q - k_q \sin V_p \cos V_q), \\ h\sin V_p = B_p + A_q(\sin V_p \cos V_q - k_q \cos V_p \sin V_q) \\ + B_q(\sin V_p \sin V_q + k_q \cos V_p \cos V_q). \end{aligned}\right\} \quad \ldots\ldots \quad (4).$$

* It is well to explain that I have sometimes elsewhere used argument to denote the argument according to the equilibrium theory, that is to say, with κ equal to zero. In this paper I call the latter the equilibrium argument.

Let

$$\left.\begin{aligned}\Sigma &= \tfrac{1}{2}\cos(V_p - V_q) + \tfrac{1}{2}\cos(V_p + V_q) = \cos V_p \cos V_q, \\ \Delta &= \tfrac{1}{2}\cos(V_p - V_q) - \tfrac{1}{2}\cos(V_p + V_q) = \sin V_p \sin V_q, \\ \sigma &= \tfrac{1}{2}\sin(V_p - V_q) + \tfrac{1}{2}\sin(V_p + V_q) = \sin V_p \cos V_q, \\ \delta &= \tfrac{1}{2}\sin(V_p - V_q) - \tfrac{1}{2}\sin(V_p + V_q) = -\cos V_p \sin V_q.\end{aligned}\right\} \quad \ldots\ldots (5).$$

Also let

$$\left.\begin{aligned}\mathrm{F} &= \Sigma + k_q\Delta, & \mathrm{f} &= \sigma + k_q\delta, \\ \mathrm{G} &= -\delta - k_q\sigma, & \mathrm{g} &= \Delta + k_q\Sigma.\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots (6).$$

Then our equations are—

$$\left.\begin{aligned}h\cos V_p &= A_p + \mathrm{F}A_q + \mathrm{G}B_q, \\ h\sin V_p &= B_p + \mathrm{f}A_q + \mathrm{g}B_q.\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots (7).$$

A similar pair of equations will result from each H. and L.W. When a series of tides is considered, we may take the mean of the equations and substitute a mean F, G, f, g.

The general principle here adopted is to take the means over such periods that the mean F, G, f, g become very small. In fact, we shall, in several cases, be able to reduce them so far that these terms are negligible, and get simply $\frac{1}{n+1}\Sigma h \frac{\cos}{\sin} V_p = \frac{A_p}{B_p}$; but in other cases, where what is typified as the p tide is a small one, whilst one or more of the tides typified as q is large, it will be necessary to find F, G, f, g. The finding of these coefficients is clearly reducible to the finding of the mean values of $\frac{\cos}{\sin}(V_p \pm V_q)$.

Another useful principle may be illustrated thus: if the q tide does not differ much in speed from the p tide, we may put $V_q = V_p + \nu t$, where ν is a small speed. Then we write

$$\begin{aligned}h &= R_p\cos(V_p - \zeta_p) + R_q\cos(V_p + \nu t - \zeta_q) \\ &= \cos V_p\{R_p\cos\zeta_p + R_q\cos(\nu t - \zeta_q)\} \\ &\quad + \sin V_p\{R_p\sin\zeta_p - R_q\sin(\nu t - \zeta_q)\}.\end{aligned}$$

If we neglect ν/p, the condition for maximum and minimum in conjunction with this gives

$$\begin{aligned}h\cos V_p &= R_p\cos\zeta_p + R_q\cos(\nu t - \zeta_q), \\ h\sin V_q &= R_p\sin\zeta_p - R_q\sin(\nu t - \zeta_q).\end{aligned}$$

Then taking the mean of these equations over a period beginning with $t = 0$ and ending when $t = \pi/\nu$, we have (writing $A_p = R_p \cos \zeta_p$, $B_p = R_p \sin \zeta_p$)

$$\frac{1}{n+1}\Sigma h \cos V_p = A_p + \lambda R_q \cos(\alpha - \zeta_q),$$

$$\frac{1}{n+1}\Sigma h \sin V_p = B_p - \lambda R_q \sin(\alpha - \zeta_q),$$

where λ and α are certain constants, depending on the sum of a trigonometrical series.

Again, if we take means from $t = \pi/\nu$ to $t = 2\pi/\nu$, the second terms have their signs changed.

Hence the difference between these two successive sums will give $\lambda R_q \cos(\alpha - \zeta_q)$ and $\lambda R_q \sin(\alpha - \zeta_q)$. There will be usually two terms such as those typified by q, and we shall then have to take two other means, viz., one beginning at $\pi/2\nu$ and ending at $3\pi/2\nu$, and the other beginning at $3\pi/2\nu$ and ending at $5\pi/2\nu$. From the difference of these sums we get $-\lambda R_q \sin(\alpha - \zeta_q)$ and $\lambda R_q \cos(\alpha - \zeta_q)$. From these four equations the two R_q's and the two ζ_q's are found. The solution is a little complicated in reality by the fact that it is not possible to take $t = 0$ exactly at the beginning of the series, because the first tide does not occur exactly at noon, but this is a detail which will become clear below.

When all the A's and B's or R's and ζ's have been found, the position of the sun and moon at the epoch, found from the Nautical Almanac, and certain constants found from the Auxiliary Tables in Baird's 'Manual of Tidal Observations,'* are required to complete the evaluation of the H's and κ's.

The details of the processes will become clear when we consider the various tides.

It may be worth mentioning that I have almost completely evaluated the F's and G's, which give the perturbation of one tide on another, in the case considered in the Appendix. Without giving any of the details of the laborious arithmetic involved, it may suffice to say that the conclusion fully justifies the omission of all those terms, which are neglected in the computation as presented below.

§ 4. *The tides* N *and* L.

These are the two lunar elliptic tides.

For the sake of brevity all the tides excepting M_2, N, L are omitted from the analytical expressions.

Since $\qquad V_n = V_m - (\sigma - \varpi)t, \quad V_l = V_m + (\sigma - \varpi)t,$

* Taylor and Francis, Fleet Street, 1886.

the expression becomes

$$h = A_m \cos V_m + B_m \sin V_m + R_n \cos [V_m - (\sigma - \varpi)t - \zeta_n] \\ + R_l \cos [V_m + (\sigma - \varpi)t - \zeta_l],$$

$$= \cos V_m \{A_m + R_m \cos [(\sigma - \varpi)t + \zeta_n] + R_l \cos [(\sigma - \varpi)t - \zeta_l]\}$$

$$+ \sin V_m \{B_m + R_m \sin [(\sigma - \varpi)t + \zeta_n] - R_l \sin [(\sigma - \varpi)t - \zeta_l]\}.$$

Hence, taking into account the equation which expresses that h is a maximum or minimum, and neglecting the variation of $s-p$ compared with that of V_m, we have—

$$\left.\begin{aligned} h \cos V_m &= A_m + R_n \cos [(\sigma - \varpi)t + \zeta_n] + R_l \cos [(\sigma - \varpi)t - \zeta_l], \\ h \sin V_m &= B_m + R_n \sin [(\sigma - \varpi)t + \zeta_n] - R_l \sin [(\sigma - \varpi)t - \zeta_l] \end{aligned}\right\} \dots\dots (8).$$

The mean interval between each tide and the next is 6·210 hours. Then if e be the increment of $s-p$ in that period (so that with $\sigma-\varpi$ equal to $0^\circ\cdot54437$ per hour, e is equal to $3^\circ\cdot3807$), and if a, b be the values of $(\sigma-\varpi)t+\zeta_n$ and $(\sigma-\varpi)t-\zeta_l$ at the time of the first tide under consideration, the equations corresponding to the $(r+1)^{\text{th}}$ tide are approximately—

$$\left.\begin{aligned} h \cos V_m &= A_m + R_n \cos (a+re) + R_l \cos (b+re), \\ h \sin V_m &= B_m + R_n \sin (a+re) - R_l \sin (b+re) \end{aligned}\right\} \dots\dots (9).$$

If we take the mean of $n+1$ successive tides, the two latter terms on the right of (9) will be multiplied by $\dfrac{\sin \frac{1}{2}(n+1)e}{(n+1) \sin \frac{1}{2}e}$, and the r in the arguments $a+re$, $b+re$, will be equal to $\frac{1}{2}n$. If the $(n+2)^{\text{th}}$ tide falls exactly a semi-lunar-anomalistic period later than the first, $(n+1)e = \pi$. On account of the incommensurability of the angular velocity $\sigma-\varpi$ this condition cannot be rigorously satisfied, but if the whole series of observations be broken up into such semi-periods, then on the average of many such summations it may be taken as true.

Then, since $\frac{1}{2}e$ is a small angle,

$$(n+1) \sin \tfrac{1}{2}e = \tfrac{1}{2}\pi, \text{ and } \sin \tfrac{1}{2}(n+1)e = 1;$$

hence the factor is $2/\pi$.

Again $\frac{1}{2}ne = \frac{1}{2}\pi - \frac{1}{2}e$; thus, if $n+1$ is the mean number of tides in a semi-anomalistic period, our mean equations are—

$$\left.\begin{aligned} \frac{\pi}{2(n+1)}\{\Sigma h \cos V_m - A_m\} &= -R_n \sin (a-\tfrac{1}{2}e) - R_l \sin (b-\tfrac{1}{2}e), \\ \frac{\pi}{2(n+1)}\{\Sigma h \sin V_m - B_m\} &= R_n \cos (a-\tfrac{1}{2}e) - R_l \cos (b-\tfrac{1}{2}e), \end{aligned}\right\} (10),$$

where the summations Σ are carried out over the first semi-lunar-anomalistic period, which may be designated as 1.

In applying these equations to the next semi-period 2, the result is got by writing $a+(n+1)e$ or $a+\pi$ for a, and $b+\pi$ for b.

Thus the equations are simply the same as (10), with the signs on the *left* changed.

The equations for semi-periods 3, 4, &c., will be all identical on the right, with alternately $+$ and $-$ signs on the left.

Let the observations run over m semi-lunar-anomalistic periods; then double the equations appertaining to periods 2, 3, . . . $(m-1)$, and add all the m equations together, and divide by $2(m-1)$, and we have—

$$\left.\begin{aligned}\frac{\pi}{4(n+1)(m-1)}\Sigma h\cos V_m &= -R_n\sin(a-\tfrac{1}{2}e)-R_l\sin(b-\tfrac{1}{2}e),\\ \frac{\pi}{4(n+1)(m-1)}\Sigma h\sin V_m &= R_n\cos(a-\tfrac{1}{2}e)-R_l\cos(b-\tfrac{1}{2}e),\end{aligned}\right\}\ldots (11),$$

where Σ now denotes summation of the following kind:—

$$\{\Sigma(1)-\Sigma(2)\}+\{\Sigma(3)-\Sigma(2)\}+\{\Sigma(3)-\Sigma(4)\}+\{\Sigma(5)-\Sigma(4)\}+\&c.,$$

the numbers (1), (2), &c., indicating the number of the semi-lunar-anomalistic-periods over which the partial sums are taken.

Suppose the whole series of observations to be reduced covers $2m+1$ *quarter*-lunar-anomalistic periods, which we denote by i, ii, iii, &c.

First suppose that the semi-period denoted previously by 1 consists of i + ii, that 2 consists of iii + iv, and so on.

Let t_0 be the time of the first tide of the series, and since we take noon of the first day as epoch, t_0 cannot be more than a few hours.

Let $\quad j=\frac{1}{2}e-(\sigma-\varpi)t_0=1^\circ\cdot 6903-(\sigma-\varpi)t_0$, a small angle.

Then

$$\left.\begin{aligned}a-\tfrac{1}{2}e &= (\sigma-\varpi)t_0+\zeta_n-\tfrac{1}{2}e = \zeta_n-j,\\ b-\tfrac{1}{2}e &= (\sigma-\varpi)t_0-\zeta_l-\tfrac{1}{2}e = -(\zeta_l+j).\end{aligned}\right\}\ldots\ldots\ (12).$$

Then denoting the operation $\dfrac{\pi}{4(n+1)(m-1)}\,\Sigma$ by S° (the mark ° indicating that the first tide included is nearly at epoch, when $(\sigma-\varpi)t=0$), we have from (11) and (12)

$$\left.\begin{aligned}S^\circ h\cos V_m &= -R_n\sin(\zeta_n-j)+R_l\sin(\zeta_l+j),\\ S^\circ h\sin V_m &= R_n\cos(\zeta_n-j)-R_l\cos(\zeta_l+j).\end{aligned}\right\}\ldots\ldots\ (13).$$

Secondly, suppose the semi-lunar-anomalistic period indicated by 1 consists of ii + iii, that 2 consists of iv + v, and so on.

Obviously the result is got by writing $t_0+\frac{1}{2}\pi/(\sigma-\varpi)$ for t_0, or, what amounts to the same thing, by putting $j-\frac{1}{2}\pi$ in place of j; but we must also write $S^{\frac{1}{2}\pi}$ for S°, so as to show that the summation begins when $(\sigma-\varpi)t$ is nearly equal to $\frac{1}{2}\pi$. Then—

$$\left.\begin{aligned} S^{\frac{1}{2}\pi}h\cos V_m &= -R_n\cos(\zeta_n-j)-R_l\cos(\zeta_l+j), \\ S^{\frac{1}{2}\pi}h\sin V_m &= -R_n\sin(\zeta_n-j)-R_l\sin(\zeta_l+j). \end{aligned}\right\}\ldots\ldots \quad (14).$$

Hence

$$\left.\begin{aligned} R_n\sin(\zeta_n-j) &= -S^{\circ}h\cos V_m-S^{\frac{1}{2}\pi}h\sin V_m, \\ R_n\cos(\zeta_n-j) &= S^{\circ}h\sin V_m-S^{\frac{1}{2}\pi}h\cos V_m, \\ R_l\sin(\zeta_l+j) &= S^{\circ}h\cos V_m-S^{\frac{1}{2}\pi}h\sin V_m, \\ R_l\cos(\zeta_l+j) &= -S^{\circ}h\sin V_m-S^{\frac{1}{2}\pi}h\cos V_m. \end{aligned}\right\}\ldots\ldots\ldots \quad (15).$$

These four equations give the four unknowns R_n, ζ_n, R_l, ζ_l, and j is equal to $1^{\circ}\cdot69-(\sigma-\varpi)t_0$.

Then if u_n, u_l denote the equilibrium arguments of the tides N and L at epoch, we have—

$$u_n = 2(h_0-\nu)-2(s_0-\xi)-(s_0-p_0),$$

$$u_l = 2(h_0-\nu)-2(s_0-\xi)+(s_0-p_0)+\pi,$$

where h_0, s_0, p_0 are the mean longitudes of moon, sun, and lunar perigee at epoch, and ν and ξ are small angles, functions of the longitude of the moon's node (tabulated in Baird's Manual).

Then if f_m is the factor of reduction (also tabulated by Baird) for the tides M_2, N, L,

$$\kappa_n = \zeta_n+u_n, \qquad \kappa_l = \zeta_l+u_l,$$

$$H_n = \frac{R_n}{f_m}, \qquad H_l = \frac{R_l}{f_m}.$$

In this investigation the interferences of the solar and diurnal tides are neglected, on the assumption that they are completely eliminated.

The difference between a lunar period and an anomalistic period is so small that the elimination of the diurnal tides will be satisfactory, but the effect of the solar tide will probably be sensible, unless we have under reduction 13 quarter-lunar-anomalistic periods, which only exceed 6 semi-lunations by about 25 hours.

The evaluation of the elliptic tides N and L from a series of observations shorter than a quarter year would be very unsatisfactory, and

it is not likely that such an evaluation will be attempted. But if such a case is undertaken, the solar disturbance may be found by a plan strictly analogous to that pursued below in the case of the tides K_1, O, P. The reader may be left to deduce the requisite formulæ from the theory in § 3.

In the case of a long series of observations, each quarter year should be reduced independently, and the mean values of $H_n \cos \kappa_n$ and $H_n \sin \kappa_n$ should be adopted as the values of the functions; whence H_n and κ_n are easily found. The L tide is, of course, to be treated similarly.

§ 5. *The Tide* M_2.

This is the principal lunar tide.

If we take the mean of $n+1$ successive tides, the equations (9) give us approximately—

$$\frac{1}{n+1}\Sigma h \cos V_m = A_m, \qquad \frac{1}{n+1}\Sigma h \sin V_n = B_m \ldots . (16).$$

We here assume that in taking this mean over an exact number of semi-lunations, the lunar elliptic tides, the solar tides, and the diurnal tides are eliminated.

With respect to the elliptic tides, this condition can only be approximately satisfied, because no small number of semi-lunations is equal to a number of anomalistic periods, and the like is true of the diurnal tides. In the example given below the diurnal tides are much larger than the elliptic tides, and I have found by actual computation (the details of which are not, however, given) that the disturbance in the value of the M_2 tide arising from the diurnal tides is quite insensible, and it may be safely accepted that the same is true of the disturbance from the elliptic tides.

With respect to the disturbance arising from the principal solar tide S_2, I find that it is adequately, although not completely, eliminated by making the number $n+1$ of tides under summation Σ cover an exact number of semi-lunations.

If the whole series of observations be short, it would be pedantic to attempt a close accuracy in results, and we may accept these formulæ; if the series be long, the residual errors will be gradually completely eliminated.

We have then—

$$R_m \cos \zeta_m = A_m, \qquad R_m \sin \zeta_m = B_m.$$

If u_m be the equilibrium argument at epoch, we have

$$u_m = 2(h_0 - \nu) - 2(s_0 - \xi).$$

Whence $$\kappa_m = \zeta_m + u_m, \text{ and } H_m = \frac{R_m}{f_m}.$$

The meanings of h_0, s_0, ν, ξ, f_m, have been explained in the last section.

§ 6. *The Tides* S_2 *and* K_2.

These are the principal solar and luni-solar semi-diurnal tides.

If the tide S_2 is in the same phase as K_2 at any time, three months later they are in opposite phases. Hence, for a short series of observations, the two tides cannot be separated, and both must be considered together. It is proposed to treat a long series of observations as made up of a succession of short series; hence I begin with a short series.

For the sake of brevity all the tides excepting S_2 and K_2 are omitted from the analytical expressions.

Since $\qquad V'' = V_s + 2\eta t,$

$$h = R_s \cos(V_s - \zeta_s) + R'' \cos(V_s + 2\eta t - \zeta''),$$

$$= \cos V_s \{R_s \cos \zeta_s + R'' \cos(2\eta t - \zeta'')\}$$

$$+ \sin V_s \{R_s \sin \zeta_s - R'' \sin(2\eta t - \zeta'')\}.$$

Hence, taking into account the equation which expresses that h is a maximum or minimum, and neglecting the variation of $2h$ or $2\eta t$ compared with that of V_s, we have—

$$h \cos V_s = R_s \cos \zeta_s + R'' \cos(2\eta t - \zeta''),$$

$$h \sin V_s = R_s \sin \zeta_s - R'' \sin(2\eta t - \zeta'').$$

The mean interval between each tide and the next is $6^{h}\cdot 210$. Then if g be the increment of $2h$ in that period (so that with 2η equal to $0^{\circ}\cdot 082$ per hour, g is equal to $0^{\circ}\cdot 510$), the equations corresponding to the $(r+1)^{\text{th}}$ tide are approximately—

$$\left.\begin{aligned} h \cos V_s &= R_s \cos \zeta_s + R'' \cos(rg - \zeta''), \\ h \sin V_s &= R_s \sin \zeta_s - R'' \sin(rg - \zeta'') \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (17).$$

Now, if P be the cube of the ratio of the sun's parallax to its mean parallax, the expression for S_2, together with its parallactic inequality (the tides T, R of harmonic notation), is $PH_s \cos(2t - \kappa_s)$.

Since t is the mean solar hour angle, 2t is the same thing as V_s.

Hence $\qquad R_s = PH_s, \quad \zeta_s = \kappa_s.$

Also if P_0 be the value of P at epoch, then for a period of two or three months we may take $P = P_0(1 + pt)$, where $P_0 p$ is equal to dP/dt.

Again, if we put $\gamma = \frac{\mathrm{H}''}{\mathrm{H}_s}$, we have

$$R'' = \mathrm{f}''\mathrm{H}'' = \mathrm{f}''\gamma\mathrm{H}_s.$$

Also since the argument of the K_2 tide is $2\mathrm{t}+2h-2\nu''-\kappa''$, where $2\nu''$ is a certain function of the longitude of the moon's node (tabulated by Baird), and since $\mathrm{t} = 0$, $h = h_0$ at epoch, it follows that

$$-\zeta'' = 2h_0-2\nu''-\kappa''.$$

Now, when the means of the equations (17) are taken for $n+1$ successive tides, the latter terms become $\frac{\lambda_n}{\gamma}\mathrm{R}''\,{}^{\cos}_{\sin}(\tfrac{1}{2}ng-\zeta'')$, where

$$\lambda_n = \gamma\cdot\frac{\sin\frac{1}{2}(n+1)g}{(n+1)\sin\frac{1}{2}g} \quad\text{................ (18).}$$

Also, if we write

$$\left.\begin{aligned} \omega &= 2h_0-2\nu''+\tfrac{1}{2}ng, \\ \Pi &= P_0(1+\tfrac{1}{2}np\times 6^{\mathrm{h}}\cdot 21), \\ A_s &= \frac{1}{n+1}\Sigma h\cos V_s, \\ B_s &= \frac{1}{n+1}\Sigma h\sin V_s \end{aligned}\right\} \quad\text{............ (19),}$$

our equations become—

$$\left.\begin{aligned} A_s &= \Pi\mathrm{H}_s\cos\kappa_s+\mathrm{f}''\lambda_n\mathrm{H}_s\cos(\omega-\kappa''), \\ B_s &= \Pi\mathrm{H}_s\sin\kappa_s-\mathrm{f}''\lambda_n\mathrm{H}_s\sin(\omega-\kappa''). \end{aligned}\right\} \quad\text{........ (20).}$$

It may be observed that Π is the mean value of P during the interval embraced by the $n+1$ tides.

In reducing a short series of observations we have to assume what is usually nearly true, viz., that $\kappa'' = \kappa_s$ and $\gamma = 0{\cdot}272$, as would be the case in the equilibrium theory of tides.

With this hypothesis, put

$$\begin{aligned} U\cos\phi &= \Pi+\lambda_n\mathrm{f}''\cos\omega, \\ U\sin\phi &= \phantom{\Pi+{}}\lambda_n\mathrm{f}''\sin\omega, \end{aligned}$$

from which to find U and ϕ. Then

$$\begin{aligned} A_s &= \mathrm{H}_sU\cos(\kappa_s-\phi), \\ B_s &= \mathrm{H}_sU\sin(\kappa_s-\phi), \end{aligned}$$

from which to find H_s and κ_s.

Lastly, $\kappa'' = \kappa_s$, $\mathrm{H}'' = \gamma\mathrm{H}_s = 0{\cdot}272\ \mathrm{H}_s$.

In order to minimise the disturbance due to the lunar tide M_2, we have to make the $n+1$ tides cover an exact number of semi-lunations, namely, the same period as that involved in the evaluation of M_2. The elimination of the M_2 tide is adequate, although not so complete as the elimination of the effect of the S_2 tide on M_2, because M_2 is nearly three times as large as S_2.

A Long Series of Observations.—Suppose that there is a half year of observations, or two periods of six semi-lunations, each of which periods contains exactly the same number of tides.

Then each of these periods is to be reduced independently with the assumption that $\gamma = 0{\cdot}272$ and $\kappa_s = \kappa''$. If this assumption is found subsequently to be very incorrect, it might be necessary to amend these reductions by multiplying λ_n by $\mathrm{H}'' \div 0{\cdot}272\mathrm{H}_s$, and by adding $\kappa_s - \kappa''$ to ω; but such repetition will not usually be necessary. From these reductions we get independent values of $\mathrm{H}_s \cos \kappa_s$, $\mathrm{H}_s \sin \kappa_s$ from each quarter year, and the mean of these is to be adopted, from which to compute H_s and κ_s. It remains to evaluate H'' and κ''.

The factor f'' and the angle $2\nu''$ vary so slowly that the change may be neglected from one quarter to the next, although each quarter is supposed to have been reduced with its proper values.

Let h_0 and h'_0 be the sun's mean longitude at the two epochs; they will clearly differ by nearly 90°, and we put $2h'_0 = 2h_0 + \pi + 2\delta h$. Hence it is clear that the value of ω in the second quarter is $\omega + 2\delta h + \pi$.

Thus the four equations, such as (20), appertaining to the two quarters, may be written—

$$\left.\begin{aligned}
A_s &= \Pi \mathrm{H}_s \cos \kappa_s + \frac{\lambda_n}{\gamma} \cdot \mathrm{f}''\mathrm{H}'' \cos(\omega - \kappa''), \\
B_s &= \Pi \mathrm{H}_s \sin \kappa_s - \frac{\lambda_n}{\gamma} \cdot \mathrm{f}''\mathrm{H}'' \sin(\omega - \kappa''), \\
A_s' &= \Pi' \mathrm{H}_s \cos \kappa_s - \frac{\lambda_n}{\gamma} \cdot \mathrm{f}''\mathrm{H}'' \cos(\omega + 2\delta h - \kappa''), \\
\mathrm{B}_s' &= \Pi' \mathrm{H}_s \sin \kappa_s + \frac{\lambda_n}{\gamma} \cdot \mathrm{f}''\mathrm{H}'' \sin(\omega + 2\delta h - \kappa''),
\end{aligned}\right\} \quad \ldots\ldots \quad (21),$$

where the accented symbols apply to the second quarter, and where $\dfrac{\lambda_n}{\gamma} = \dfrac{\sin \frac{1}{2}(n+1)g}{(n+1)\sin \frac{1}{2}g} = 0{\cdot}656$, a constant.

From (21),

$$A_s - A_s' - (\Pi - \Pi')\mathrm{H}_s \cos \kappa_s = 2\frac{\lambda_n}{\gamma} \cdot \mathrm{f}''\mathrm{H}'' \cos \delta h \cos(\omega + \delta h - \kappa''),$$

$$-B_s + B_s' + (\Pi - \Pi')\mathrm{H}_s \sin \kappa_s = 2\frac{\lambda_n}{\gamma} \cdot \mathrm{f}''\mathrm{H}'' \cos \delta h \sin(\omega + \delta h - \kappa'').$$

From these two equations, H'' and κ'' may be computed, and since $\Pi - \Pi'$ is very small, approximate values of $H_s \cos \kappa_s$, $H_s \sin \kappa_s$ suffice.

§ 7. *The Diurnal Tides* K_1, O, P.

Amongst the diurnal tides I shall only consider K_1 the luni-solar diurnal, O the principal lunar diurnal, and P the principal solar diurnal tides.

There is the same difficulty in separating P from K_1 as in the case of K_2 and S_2, and therefore in a short series of observations P and K_1 have to be treated together. It is proposed to treat a long series of observations as made up of a succession of short series; hence I begin with a short series.

For the sake of brevity all the tides excepting K_1, O, P are omitted from the analytical expressions.

If $\frac{1}{2}V_m$ denotes $(\gamma-\sigma)t$, we have

$$V' = \tfrac{1}{2}V_m + \sigma t,\quad V_o = \tfrac{1}{2}V_m - \sigma t,\quad V_p = \tfrac{1}{2}V_m + (\sigma - 2\eta)t, \text{ and}$$

$$\begin{aligned} h = {} & R'\cos(\tfrac{1}{2}V_m + \sigma t - \zeta') + R_o \cos(\tfrac{1}{2}V_m - \sigma t - \zeta_o) \\ & \qquad + R_p \cos(\tfrac{1}{2}V_m + (\sigma - 2\eta)t - \zeta_p), \\ = {} & \cos\tfrac{1}{2}V_m\{R'\cos(\sigma t - \zeta') + R_o\cos(\sigma t + \zeta_o) + R_p\cos((\sigma - 2\eta)t - \zeta_p)\} \\ & + \sin\tfrac{1}{2}V_m\{-R'\sin(\sigma t - \zeta') + R_o\sin(\sigma t + \zeta_o) - R_p\sin((\sigma - 2\eta)t - \zeta_p)\}. \end{aligned}$$

Hence, taking account of the equation which expresses that h is a maximum or minimum, and neglecting the variation of σt compared with that of $\frac{1}{2}V_m$,* we have—

$$\begin{aligned} h\cos\tfrac{1}{2}V_m &= R'\cos(\sigma t - \zeta') + R_o\cos(\sigma t + \zeta_o) + R_p\cos((\sigma - 2\eta)t - \zeta_p), \\ h\sin\tfrac{1}{2}V_m &= -R'\sin(\sigma t - \zeta') + R_o\sin(\sigma t + \zeta_o) - R_p\sin((\sigma - 2\eta)t - \zeta_p). \end{aligned}$$

The mean interval between each tide and the next is $6^h{\cdot}210$.

Then if e be the increment of s, and z the increment of $s-2h$ in that period (so that with σ equal to $0^\circ{\cdot}5490$ per hour and $\sigma - 2\eta$ equal to $0^\circ{\cdot}4669$ per hour, e is equal to $3^\circ{\cdot}4095$ and z equal to $2^\circ{\cdot}8994$); and if a, b, c denote the values of $\sigma t - \zeta'$, $\sigma t + \zeta_o$, $(\sigma - 2\eta)t - \zeta_p$ at the time of the first tide under consideration, the equations corresponding to the $(r+1)^{\text{th}}$ tide are approximately—

$$\left.\begin{aligned} h\cos\tfrac{1}{2}V_m &= R'\cos(a + re) + R_o\cos(b + re) + R_p\cos(c + rz), \\ h\sin\tfrac{1}{2}V_m &= -R'\sin(a + re) + R_o\sin(b + re) - R_p\sin(c + rz). \end{aligned}\right\} \ldots (22).$$

If we take the mean of $n+1$ successive tides, the first pair of terms

* I have satisfied myself by analysis, which I do not reproduce, that on taking means this error becomes very small.

will be multiplied by $\dfrac{\sin \frac{1}{2}(n+1)e}{(n+1)\sin \frac{1}{2}e}$ and the last term by the similar function with z in place of e; also the r in the arguments must be put equal to $\frac{1}{2}n$.

If the $(n+2)^{\text{th}}$ tide falls exactly a semi-lunar period later than the first, $(n+1)e = \pi$. On account of the incommensurability of the angular velocity σ, this condition cannot be rigorously satisfied, but if the whole series of observations be broken up into such semi-periods, then, on the average of many such summations, it may be taken as true.

Since $\frac{1}{2}e$ is a small angle, $(n+1)\sin \frac{1}{2}e = \frac{1}{2}\pi$, and $\sin \frac{1}{2}(n+1)e = 1$; hence the first factor is equal to $2/\pi$.

Again,

$$\tfrac{1}{2}(n+1)z = \tfrac{1}{2}(n+1)e \cdot \frac{z}{e} = \tfrac{1}{2}\pi \cdot \frac{\sigma-2\eta}{\sigma} = 76^\circ\ 32' \text{ in degrees;}$$

and $(n+1)\sin \frac{1}{2}z = \frac{1}{2}\pi \cdot \dfrac{\sigma-2\eta}{\sigma}$.

Therefore

$$\frac{\sin \frac{1}{2}(n+1)z}{(n+1)\sin \frac{1}{2}z} = \frac{2}{\pi} \cdot \frac{\sigma}{\sigma-2\eta} \sin 76^\circ\ 32' = \frac{2}{\pi} \times 1{\cdot}1436 = \frac{2}{\pi} \times \lambda, \text{ suppose.}$$

Again $\quad \frac{1}{2}ne = \frac{1}{2}\pi - \frac{1}{2}e = \frac{1}{2}\pi - 1^\circ{\cdot}7048,$

$$\tfrac{1}{2}nz = \tfrac{1}{2}\pi - 13^\circ{\cdot}4647 - 1^\circ{\cdot}4497 = \tfrac{1}{2}\pi - 14^\circ{\cdot}9144.$$

Now let

$$\left.\begin{aligned} \alpha &= a - \ \ 1^\circ{\cdot}7048, \\ \beta &= b - \ \ 1^\circ{\cdot}7048, \\ \gamma &= c - 14^\circ{\cdot}9144, \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots \quad (23),$$

and we have

$$\left.\begin{aligned} a + \tfrac{1}{2}ne &= \tfrac{1}{2}\pi + \alpha, \\ b + \tfrac{1}{2}ne &= \tfrac{1}{2}\pi + \beta, \\ c + \tfrac{1}{2}nz &= \tfrac{1}{2}\pi + \gamma. \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots \quad (24).$$

Thus, if $n+1$ is the mean number of tides in a semi-lunar period, the means of equations (22) become

$$\left.\begin{aligned} \frac{\pi}{2(n+1)} \Sigma h \cos \tfrac{1}{2}V_m &= -R' \sin \alpha - R_0 \sin \beta - \lambda R_p \sin \gamma, \\ \frac{\pi}{2(n+1)} \Sigma h \sin \tfrac{1}{2}V_m &= -R' \cos \alpha + R_0 \cos \beta - \lambda R_p \cos \gamma, \end{aligned}\right\} \ldots \quad (25),$$

where the summations are carried out over the first semi-lunar period, which may be designated as 1.

In applying these equations to the next semi-period 2, the result is obtained by writing $a+(n+1)e$ for a, $b+(n+1)e$ for b, and $c+(n+1)z$ for c; that is to say, $a+\pi$ for a, $b+\pi$ for b, and $c+153^\circ\cdot0706$ or $c+\pi-26^\circ\cdot9294$ for c.

If, therefore, we put $\epsilon = 26^\circ\cdot9294$, we obtain the result from (25) by changing the signs on the left and writing $\gamma-\epsilon$ for γ.

The equations for semi-periods 3, 4, 5, &c., will be alternately $+$ and $-$ on the left, and identical as regards the terms in α and β, but with $\gamma-2\epsilon$, $\gamma-3\epsilon$, $\gamma-4\epsilon$, &c., successively in place of γ.

Let the observations run over m semi-lunar periods; then double the equations appertaining to periods 2, 3 . . . $(m-1)$, add all the m equations together, and divide by $2(m-1)$.

The terms in R_p will involve the series

$$\frac{\sin}{\cos}\gamma+2\frac{\sin}{\cos}(\gamma-\epsilon)+2\frac{\sin}{\cos}(\gamma-2\epsilon)+\ldots+\frac{\sin}{\cos}(\gamma-(m-1)\epsilon).$$

This is equal to

$$2\frac{\sin\frac{1}{2}(m-1)\epsilon}{\tan\frac{1}{2}\epsilon}\frac{\sin}{\cos}(\gamma-\tfrac{1}{2}(m-1)\epsilon).$$

Then if we put

$$\mu=\frac{\lambda\sin\frac{1}{2}(m-1)\epsilon}{(m-1)\tan\frac{1}{2}\epsilon},\quad \text{where } \lambda = 1\cdot1436,$$

our equations (25) become

$$\left.\begin{aligned}&\frac{\pi}{4(n+1)(m-1)}\Sigma h\cos\tfrac{1}{2}V_m\\&\qquad=-R'\sin\alpha-R_0\sin\beta-\mu R_p\sin(\gamma-\tfrac{1}{2}(m-1)\epsilon),\\&\frac{\pi}{4(n+1)(m-1)}\Sigma h\sin\tfrac{1}{2}V_m\\&\qquad=-R'\cos\alpha+R_0\cos\beta-\mu R_p\cos(\gamma-\tfrac{1}{2}(m-1)\epsilon),\end{aligned}\right\}\ldots (26),$$

where Σ now denotes summation of the following kind:—

$$\{\Sigma(1)-\Sigma(2)\}+\{\Sigma(3)-\Sigma(2)\}+\{\Sigma(3)-\Sigma(4)\}+\ldots$$

Suppose the whole series of observations to be reduced covers exactly $2m+1$ *quarter*-lunar periods, which we denote by I, II, III, &c.

First suppose that the semi-period denoted previously by 1 consists of I+II, that 2 consists of III+IV, and so on.

Let t_0 denote the time of the first tide of the series, and since noon of the first day is epoch, t_0 cannot be more than a few hours.

$$\left.\begin{aligned}&\text{Let} && i=\tfrac{1}{2}e-\sigma t_0=1^\circ\cdot7048-\sigma t_0,\\&\text{and} && k=\tfrac{1}{2}z-(\sigma-2\eta)t_0=1^\circ\cdot4497-(\sigma-2\eta)t_0;\end{aligned}\right\}\ldots\ldots (27),$$

i and k are clearly small angles.

Since $\epsilon = 26^\circ\cdot 9294$, $\frac{1}{2}\epsilon + 1^\circ\cdot 4497 = 14^\circ\cdot 9144$; and from (23)

$$\left.\begin{aligned} \alpha &= \sigma t_o - \zeta' - 1^\circ\cdot 7048 = -(\zeta' + i), \\ \beta &= \sigma t_o + \zeta_o - 1^\circ\cdot 7048 = \quad (\zeta_o - i), \\ \gamma &= (\sigma - 2\eta) t_o - \zeta_p - 14^\circ\cdot 9144 = -\zeta_p - k - \tfrac{1}{2}\epsilon \end{aligned}\right\} \dots\dots (28).$$

If the same notation be adopted as that explained in § 4 (the only difference being that we now deal with quarter-lunar instead of quarter-anomalistic periods), we have

$$S^\circ h \cos \tfrac{1}{2} V_m = R' \sin(\zeta' + i) - R_o \sin(\zeta_o - i) + \mu R_p \sin(\zeta_p + k + \tfrac{1}{2} m\epsilon),$$

$$S^\circ h \sin \tfrac{1}{2} V_m = -R' \cos(\zeta' + i) + R_o \cos(\zeta_o - i) - \mu R_p \cos(\zeta_p + k + \tfrac{1}{2} m\epsilon), \quad \dots (29).$$

Secondly, suppose the semi-lunar period indicated by 1 consists of II + III, that 2 consists of IV + V, and so on. Then, obviously, the result is got by writing $t_o + \frac{1}{2}\pi/\sigma$ for t_o; that is to say, write $i - \frac{1}{2}\pi$ for i and $k - \frac{1}{2}(\sigma - 2\eta)\pi/\sigma$, or $k - \frac{1}{2}\pi + \eta\pi/\sigma$ for k. But $\eta\pi/\sigma$ is equal to $\frac{1}{2}\epsilon$, and we write $k - \frac{1}{2}\pi + \frac{1}{2}\epsilon$ for k. Therefore, following the notation used in §4 for N and L,

$$\left.\begin{aligned} S^{\frac{1}{2}\pi} h \cos \tfrac{1}{2} V_m &= -R' \cos(\zeta' + i) - R_o \cos(\zeta_o - i) \\ &\qquad - \mu R_p \cos(\zeta_p + k + \tfrac{1}{2}(m+1)\epsilon), \\ S^{\frac{1}{2}\pi} h \sin \tfrac{1}{2} V_m &= -R' \sin(\zeta' + i) - R_o \sin(\zeta_o - i) \\ &\qquad - \mu R_p \sin(\zeta_p + k + \tfrac{1}{2}(m+1)\epsilon). \end{aligned}\right\} (30).$$

These four S's require correction for the disturbance due to the semi-diurnal terms M_2 and S_2, and I shall return to this point later. In the meantime write

$$\left.\begin{matrix} W \\ X \end{matrix}\right\} = S^\circ h \begin{matrix}\cos \\ \sin\end{matrix} \tfrac{1}{2} V_m + \text{corr.}, \qquad \left.\begin{matrix} Y \\ Z \end{matrix}\right\} = S^{\frac{1}{2}\pi} h \begin{matrix}\cos \\ \sin\end{matrix} \tfrac{1}{2} V_m + \text{corr.} \quad (31),$$

and we have—

$$\left.\begin{aligned} \tfrac{1}{2}(W + Z) &= -R_o \sin(\zeta_o - i) - \mu R_p \sin \tfrac{1}{4}\epsilon \cos(\zeta_p + k + \tfrac{1}{4}(2m+1)\epsilon), \\ \tfrac{1}{2}(X - Y) &= \quad R_o \cos(\zeta_o - i) - \mu R_p \sin \tfrac{1}{4}\epsilon \sin(\zeta_p + k + \tfrac{1}{4}(2m+1)\epsilon), \end{aligned}\right\} (32),$$

$$\left.\begin{aligned} \tfrac{1}{2}(W - Z) &= \quad R' \sin(\zeta' + i) + \mu R_p \cos \tfrac{1}{4}\epsilon \sin(\zeta_p + k + \tfrac{1}{4}(2m+1)\epsilon), \\ \tfrac{1}{2}(X + Y) &= -R' \cos(\zeta' + i) - \mu R_p \cos \tfrac{1}{4}\epsilon \cos(\zeta_p + k + \tfrac{1}{4}(2m+1)\epsilon). \end{aligned}\right\} (33).$$

If we put
$$\left.\begin{aligned} L &= \{\tfrac{1}{2}(X + Y) + R' \cos(\zeta' + i)\} \tan \tfrac{1}{4}\epsilon, \\ M &= \{\tfrac{1}{2}(W - Z) - R' \sin(\zeta' + i)\} \tan \tfrac{1}{4}\epsilon, \end{aligned}\right\} \dots\dots (34),$$

the equations (33) may be written—

$$\left.\begin{aligned}\tfrac{1}{2}(\mathrm{W}+\mathrm{Z})-\mathrm{L} &= -R_o \sin(\zeta_o - i),\\ \tfrac{1}{2}(\mathrm{X}-\mathrm{Y})+\mathrm{M} &= R_o \cos(\zeta_o - i).\end{aligned}\right\}\quad\ldots\ldots\ldots\ (35).$$

The four equations (32), (33) involve six unknown quantities, R', ζ', R_o, ζ_o, R_p, ζ_p, and are insufficient for their determination.

In reducing a short series of observations it is necessary to assume what is usually nearly true, viz., that $\kappa_p = \kappa'$, and $\mathrm{H}_p/\mathrm{H}' = 0{\cdot}3309$, as would be the case in the equilibrium theory of tides.

Then, writing q for $0{\cdot}3309$, we have approximately $R_p = \mathrm{H}_p = q\mathrm{H}'$. The argument of the K_1 tide is $\mathrm{t}+(h-\nu')-\frac{1}{2}\pi-\kappa'$, where ν' is a certain function of the longitude of the moon's node (tabulated by Baird); and the argument of the P tide is $\mathrm{t}-h+\frac{1}{2}\pi-\kappa_p$.

At the noon which is taken as epoch $\mathrm{t} = 0$, $h = h_o$, and the two arguments are equal to $-\zeta'$ and $-\zeta_p$.

Hence
$$\begin{aligned}-\zeta' &= h_o - \nu' - \tfrac{1}{2}\pi - \kappa',\\ -\zeta_p &= -h_o + \tfrac{1}{2}\pi - \kappa_p.\end{aligned}$$

Therefore
$$\zeta_p = \zeta' + 2h_o - \nu' - \pi + (\kappa_p - \kappa').$$

Putting $\kappa_p = \kappa'$ as explained above,

$$\zeta_p + k = \zeta' + i + 2h_o - \nu' - \pi + l,$$

where
$$\begin{aligned} l = k - i &= [1^\circ{\cdot}450 - (\sigma - 2\eta)t_o] - [1^\circ{\cdot}705 - \sigma t_o],\\ &= -0^\circ{\cdot}255 + 2\eta t_o, \text{ a small angle}\ \ldots\ldots\ldots\ (36).\end{aligned}$$

Then, if
$$\left.\begin{aligned}\theta &= 2h_o - \nu' + l + \tfrac{1}{4}(2m+1)\epsilon,\\ \rho_m &= q\mu\cos\tfrac{1}{4}\epsilon,\end{aligned}\right\}\quad\ldots\ldots\ldots\ (37),$$

we have
$$\left.\begin{aligned}\tfrac{1}{2}(\mathrm{W}-\mathrm{Z}) &= \mathrm{f}'\mathrm{H}'\sin(\zeta'+i) - \rho_m\mathrm{H}'\sin(\zeta'+i+\theta),\\ \tfrac{1}{2}(\mathrm{X}+\mathrm{Y}) &= -\mathrm{f}'\mathrm{H}'\cos(\zeta'+i) + \rho_m\mathrm{H}'\cos(\zeta'+i+\theta).\end{aligned}\right\}\ldots\ (38).$$

Let
$$\left.\begin{aligned}\mathrm{T}\cos\psi &= \mathrm{f}' - \rho_m\cos\theta,\\ \mathrm{T}\sin\psi &= \phantom{\mathrm{f}' -{}} \rho_m\sin\theta.\end{aligned}\right\}\quad\ldots\ldots\ldots\ (39),$$

whence T and ψ may be computed; and

$$\left.\begin{aligned}\tfrac{1}{2}(\mathrm{W}-\mathrm{Z}) &= \mathrm{H}'\mathrm{T}\sin(\zeta'+i-\psi),\\ \tfrac{1}{2}(\mathrm{X}+\mathrm{Y}) &= -\mathrm{H}'\mathrm{T}\cos(\zeta'+i-\psi).\end{aligned}\right\}\quad\ldots\ldots\ldots\ (40).$$

From these we compute H' and ζ' and $\zeta'+i$.

Then if $u' = h_o - \nu' - \frac{1}{2}\pi$, the equilibrium argument at epoch of K_1,

$$\kappa' = \zeta' + u'.$$

We have also $H_p = qH' = 0{\cdot}3309\ H'$, $\kappa_p = \kappa'$.

Returning to equations (34) and (35), we compute $R' = f'H'$, and hence $R' \begin{smallmatrix}\cos\\ \sin\end{smallmatrix} (\zeta' + i)$, and then L and M.

Having these, we compute R_o and ζ_o from (35).

Then, if $u_o = h_o - \nu - 2(s_o - \xi) + \frac{1}{2}\pi$, the equilibrium argument at epoch of O,

$$\kappa_o = \zeta_o + u_o, \text{ and } R_o = \frac{H_o}{f_o},$$

where f_o is a certain function of the longitude of the moon's node, tabulated in Baird's Manual.

A Long Series of Observations.—Suppose that there is a half year of observation, or two periods of thirteen quarter-lunar periods, each of which contains exactly the same number of tides.

Then each of these periods is to be reduced independently with the assumption that $q = 0{\cdot}3309$ and $\kappa_p = \kappa'$. If this assumption be found subsequently to be very incorrect, it might be necessary to amend these reductions by adding $\kappa_p - \kappa'$ to the value of θ, and by multiplying ρ_m by $H_p \div 0{\cdot}3309\ H'$, but such repetition will not usually be necessary.

From these reductions we get independent values of $H' \cos \kappa'$, $H' \sin \kappa'$, $H_o \cos \kappa_o$, $H_o \sin \kappa_o$ from each quarter year, and the means of these are to be adopted from which to compute H', κ', H_o, κ_o.

It remains to evaluate H_p and κ_p.

The factor f' and the angle ν' vary so slowly that the change from one quarter to the next may be neglected, although each quarter is supposed to have been reduced with its proper values.

Let h_o, h'_o be the values of the sun's mean longitude at the two epochs; then since the second epoch is nearly a quarter year later than the first, h'_o will exceed h_o by about 90°.

Let $h'_o = h_o + \frac{1}{2}\pi + \delta h$, so that δh is small.

If $\zeta' + \delta\zeta'$, $\zeta_p + \delta\zeta_p$ be the values of ζ', ζ_p at the second epoch, we have $\zeta' + \delta\zeta' = -h'_o + \nu' + \frac{1}{2}\pi + \kappa'$, $\zeta' = -h_o + \nu' + \frac{1}{2}\pi + \kappa'$, and therefore $\delta\zeta' = -\frac{1}{2}\pi - \delta h$.

Again, $\zeta_p + \delta\zeta_p = h'_o - \frac{1}{2}\pi + \kappa_p$, $\zeta_p = h_o - \frac{1}{2}\pi + \kappa_p$, and therefore $\delta\zeta_p = \frac{1}{2}\pi + \delta h$.

Let $i + \delta i$, $k + \delta k$ be the values of i and k corresponding to the second epoch, and let W′, X′, Y′, Z′ be the values of those quantities in the second quarter. Then, replacing $\frac{1}{4}(2m+1)\epsilon$ by $87^\circ{\cdot}5$, since that is its value when $2m+1$ is 13, we have from (33)

$$\tfrac{1}{2}(W'-Z') = -R'\cos(\zeta'+i+\delta i-\delta h) + \mu R_p \cos\tfrac{1}{4}\epsilon\cos(\zeta_p+k+\delta k+\delta h+87^\circ\cdot 5),$$

$$\tfrac{1}{2}(X'+Y') = -R'\sin(\zeta'+i+\delta i-\delta h) + \mu R_p \cos\tfrac{1}{4}\epsilon\sin(\zeta_p+k+\delta k+\delta h+87^\circ\cdot 5),$$

$$\tfrac{1}{2}(W-Z) = R'\sin(\zeta'+i) + \mu R_p\cos\tfrac{1}{4}\epsilon\sin(\zeta_p+k+87^\circ\cdot 5),$$

$$\tfrac{1}{2}(X+Y) = -R'\cos(\zeta'+i) - \mu R_p\cos\tfrac{1}{4}\epsilon\cos(\zeta_p+k+87^\circ\cdot 5).$$

Hence

$$\left.\begin{aligned} \tfrac{1}{4}(W'-Z')-\tfrac{1}{4}(X+Y) &= R'\sin\tfrac{1}{2}(\delta i-\delta h)\sin(\zeta'+i+\tfrac{1}{2}(\delta i-\delta h)) \\ &\quad +\mu R_p\cos\tfrac{1}{4}\epsilon\cos\tfrac{1}{2}(\delta k+\delta h)\cos(\zeta_p+k+\tfrac{1}{2}(\delta k+\delta h)+87^\circ\cdot 5), \\ \tfrac{1}{4}(W-Z)+\tfrac{1}{4}(X'+Y') &= -R'\sin\tfrac{1}{2}(\delta i-\delta h)\cos(\zeta'+i+\tfrac{1}{2}(\delta i-\delta h)) \\ &\quad +\mu R_p\cos\tfrac{1}{4}\epsilon\cos\tfrac{1}{2}(\delta k+\delta h)\sin(\zeta_p+k+\tfrac{1}{2}(\delta k+\delta h)+87^\circ\cdot 5). \end{aligned}\right\} \quad (41).$$

In these equations R' is equal to f$'$H$'$ and R_p is equal to H_p.

The terms involving R' are clearly small, and approximate values of R' and ζ', as derived from the first quarter, will be sufficient to compute them. Afterwards we can compute R_p or H_p and ζ_p; then if u_p denotes $-h_0+\tfrac{1}{2}\pi$, the equilibrium argument of P at the first epoch, $\kappa_p = \zeta_p+u_p$.

The values of H_p, κ_p thus deduced ought not to differ very largely from those assumed in the two independent reductions.

The same investigation serves for the evaluation of the P tide from any two sets of observations, each consisting of thirteen quarter-lunar periods, and with a small change in the analysis we need not suppose each to consist of thirteen such periods. But the two epochs must be such that sin δh is small and cos δh is large, or the formulæ, although analytically correct, will fail in their object.

§ 8.—*The Disturbance of* K_1, O, P *due to* M_2 *and* S_2.

It has been remarked in § 7 that the diurnal tides are perturbed by the semi-diurnal. The general method has been given in § 3, by which to calculate the effect on any one tide, whose increment of argument since epoch is V_p and speed is p, due to a tide whose increment is V_q and speed q.

Since in the present instance all the diurnal tides have been consolidated into one of speed $\gamma-\sigma$, we have to calculate the effect of the tides whose speeds are $2(\gamma-\sigma)$ and $2(\gamma-\eta)$ on the tide whose speed is $\gamma-\sigma$. It follows, therefore, that the factor q/p or k_q of (3) is in the first case equal to $2(\gamma-\sigma)/(\gamma-\sigma)$ or 2, and in the second case is $2(\gamma-\eta)/(\gamma-\sigma)$ or 2·070; or $k_m = 2$, $k_s = 2{\cdot}070$.

The coefficients F, G, f, g, as due to the tide M_2 of speed $2(\gamma-\sigma)$, will be written with suffix m, and as due to the tide S_2 of speed $2(\gamma-\eta)$, with suffix s. The sums and means have also to be taken in the two ways denoted by S° and $S^{\frac{1}{2}\pi}$. Hence we have altogether to compute sixteen coefficients, which by an easily intelligible notation may be written $F_m^{(o)}$, $G_m^{(o)}$, ... $f_s^{(\frac{1}{2}\pi)}$, $g_{s(\frac{1}{2}\pi)}$.

In order to compute the sixteen coefficients, it is necessary to find the mean cosines and sines of the four following angles, viz.:—

$\frac{1}{2}V_m \pm V_m$, $\frac{1}{2}V_m \pm V_s$, and the means have to be taken in the two ways denoted S° and $S^{\frac{1}{2}\pi}$.

These means are exactly the same in form as what the means of h cos and h sin (which had to be evaluated in S° and $S^{\frac{1}{2}\pi}$) would be if all the heights were regarded as positive unity, irrespective of whether they are H.W. or L.W. Hence the same plan of computation serves here as elsewhere; the plan is explained in the following section.

By comparison of equation (7) and the definitions (31) of W, X, Y, Z in the last section, we have:—

$$\left.\begin{aligned}
W &= S^\circ h\cos\tfrac{1}{2}V_m - \{A_m F_m^{(o)} + B_m G_m^{(o)} + A_s F_s^{(o)} + B_s G_s^{(o)}\},\\
X &= S^\circ h\sin\tfrac{1}{2}V_m - \{A_m f_m^{(o)} + B_m g_m^{(o)} + A_s f_s^{(o)} + B_s g_s^{(o)}\},\\
Y &= S^{\frac{1}{2}\pi} h\cos\tfrac{1}{2}V_m - \{A_m F_m^{(\frac{1}{2}\pi)} + B_m G_m^{(\frac{1}{2}\pi)} + A_s F_s^{(\frac{1}{2}\pi)} + B_s G_s^{(\frac{1}{2}\pi)}\},\\
Z &= S^{\frac{1}{2}\pi} h\sin\tfrac{1}{2}V_m - \{A_m f_m^{(\frac{1}{2}\pi)} + B_m g_m^{(\frac{1}{2}\pi)} + A_s f_s^{(\frac{1}{2}\pi)} + B_s g_s^{(\frac{1}{2}\pi)}\}.
\end{aligned}\right\} \quad (42).$$

The four quantities A_m, B_m, A_s, B_s are known from the evaluations of the tides M_2 and S_2; whence the corrections referred to in § 7 are calculable.

§ 9. *On the Summations.*

It will be seen from the preceding sections that sums have to be found of the following functions:—

$$h\frac{\cos}{\sin}V_m,\quad h\frac{\cos}{\sin}V_s,\quad h\frac{\cos}{\sin}\tfrac{1}{2}V_m;$$

and also of

$$\frac{\cos}{\sin}\tfrac{1}{2}V_m,\quad \frac{\cos}{\sin}\tfrac{3}{2}V_m,\quad \frac{\cos}{\sin}(\tfrac{1}{2}V_m \pm V_s).$$

It is necessary to calculate the five angles $\frac{1}{2}V_m$, V_s, $\frac{1}{2}V_m \pm V_s$, and V_m, for each tide, and the reader will easily see, by the example in the Appendix, how they may be computed with considerable rapidity, by aid of an auxiliary table A.

The computation of sines and cosines and multiplication by heights, may, with sufficient accuracy, be abridged, by regarding the cosine or

sine of any angle lying within a given 5° of the circumference as equal to the cosine or sine of the middle of that 5°.

The process then consists in the grouping of the heights according to the values of their V's (V_m, V_s, $\frac{1}{2}V_m$, as the case may be). The heights in each group are then summed. Since the L.W. heights are all negative, they are treated in a separate table, and are considered as positive until their combination with the H.W. at a later stage. We shall, for the present, only speak of one of these groupings, taking it as a type of both.

Since $\frac{\cos}{\sin}(\alpha+180°) = -\frac{\cos}{\sin}\alpha$, the eighteen groups forming the 3rd quadrant may be thrown in with the 1st quadrant by a mere change of sign; and the like is true of the 4th and 2nd quadrants.

Since $\cos(180°-\alpha) = -\cos\alpha$ and $\sin(180°-\alpha) = \sin\alpha$, it follows that we have to go through the 2nd quadrant in reversed order, in order to fall in with the succession which holds in the 1st quadrant, and, moreover, the cosine changes its sign, whilst the sine does not do so. Hence the following schemes will give us the eighteen groups which all have the same cosines and sines:—

for cosines

$$(1^{\text{st}}-3^{\text{rd}})-(2^{\text{nd}}-4^{\text{th}}) \text{ reversed,}$$

for sines

$$(1^{\text{st}}-3^{\text{rd}})+(2^{\text{nd}}-4^{\text{th}}) \text{ reversed.}$$

Thus, one grouping of the heights serves for both cosines and sines, and, save for the last step, the additions are the same.

The combination of the H.W. and L.W. results is best made at the stage where 1st—3rd and 2nd—4th have been formed.

The negative signs for the L.W. results are introduced before addition to the H.W. results, and total 1st—3rd and 2nd—4th are thus formed.

After the eighteen cosine and sine total numbers are thus formed, they are to be multiplied by the cosines or sines of 2° 30′, 7° 30′, 12° 30′, ... 87° 30′. The products are then summed so as to give $\Sigma h \frac{\cos}{\sin}$.

It was noted at the beginning of this section that we also have sums of the form $\Sigma \frac{\cos}{\sin}$. These sums are obviously made by entering unity in place of each height, and, of course, not treating the L.W. as negative. Thus, where the H.W. and L.W. are combined it is not necessary to change the sign of the L.W., as was done in the combination of H.W. and L.W. for $\Sigma h \frac{\cos}{\sin}$. These summations are considerably less laborious than the others.

In the case of the tides M_2 and S_2, the division of the sums $\Sigma h \frac{\cos}{\sin}$

by the total number of entries gives the required results. But for N, L, and similarly for the diurnal tides K_1, O, P, the grouping and summations have to be broken into a number of subordinate periods, which are to be operated on to form S° and $S^{\frac{1}{2}\pi}$. The multiplication by the eighteen mean cosines and sines is best deferred to a late stage in the computation.

Thus, for example, for N and L, the quarter-lunar-anomalistic periods, i, ii, iii, &c., are treated independently, and we find $(1^{st}-3^{rd})\pm(2^{nd}-4^{th}$ reversed) for each. There are thus eighteen cosine numbers and eighteen sine numbers for each of i, ii, iii, &c.

We next form the sums two and two, i+ii, iii+iv, &c.; next find the differences (i+ii)−(iii+iv), (v+vi)−(iii+iv), &c.; add the differences together; then multiply by the eighteen cosines or sines of $2\frac{1}{2}^\circ$, $7\frac{1}{2}^\circ$, &c., and finally multiply by $\frac{\pi}{4(n+1)(m-1)}$, and so find $S^\circ h \frac{\cos}{\sin}$.

We next go through exactly the same process, but beginning with ii instead of i, and so find $S^{\frac{1}{2}\pi} h \frac{\cos}{\sin}$.

The same process applies, *mutatis mutandis*, for finding S° and $S^{\frac{1}{2}\pi} \frac{\cos}{\sin}$.

There are two cases which merit attention in particular. The sorting of heights in quarter-lunar-anomalistic periods, according to values of V_m, serves, in the first instance, for the evaluation of N and L, but it serves, secondly, to evaluate M_2, for we then simply neglect the subdivision into quarter periods and treat the whole as one series, but stop at the end of a semi-lunation.

The sorting of heights in quarter-lunar periods, according to the values of $\frac{1}{2}V_m$, also serves several purposes.

We first find from it S° and $S^{\frac{1}{2}\pi} h \frac{\cos}{\sin} \frac{1}{2}V_m$, and secondly, by merely counting the entries in each group for each quarter period, instead of adding up the heights, we arrive at S° and $S^{\frac{1}{2}\pi} \cos \frac{1}{2}V_m$. (It may be noted in passing that what is wanted, according to preceding analysis, is the sum of $\frac{\cos}{\sin}(\frac{1}{2}V_m - V_m)$, so that there will be a change of sign in the sine sum to get the desired result.)

But, besides these, S° and $S^{\frac{1}{2}\pi} \frac{\cos}{\sin}(\frac{1}{2}V_m + V_m)$ can be obtained with sufficient accuracy from the same sorting.

The angles $\frac{1}{2}V_m$ were sorted in four times eighteen groups, for each quarter-lunar period. If each angle were multiplied by three, the eighteen entries of the 1^{st} quadrant would be converted into three groups of six, lying in three quadrants, viz., I^{st}, II^{nd}, III^{rd}; the

2^{nd} quadrant is changed to IV^{th}, I^{st}, II^{nd}; the 3^{rd} to III^{rd}, IV^{th}, I^{st}; and the 4^{th} to II^{nd}, III^{rd}, IV^{th}. Hence eighteen entries of $1^{st}-3^{rd}$ are converted into three sixes, $I^{st}-III^{rd}$, $II^{nd}-IV^{th}$, $-\{I^{st}-III^{rd}\}$; and eighteen entries of $2^{nd}-4^{th}$ are converted into three sixes, $-\{II^{nd}-IV^{th}\}$, $I^{st}-III^{rd}$, $II^{nd}-IV^{th}$.

Hence a new $I^{st}-III^{rd}$ of six entries is made up thus:—

first six of former $1^{st}-3^{rd}$

+second six of former $2^{nd}-4^{th}$

—third six of former $1^{st}-3^{rd}$.

And a new $II^{nd}-IV^{th}$ of six entries is made up of

—first six of former $2^{nd}-4^{th}$

+second six of former $1^{st}-3^{rd}$

+third six of former $2^{nd}-4^{th}$.

These $I^{st}-III^{rd}$ and $II^{nd}-IV^{th}$ may now be treated just like the other ones. Thus, without calculating $\frac{4}{3}V_m$, we have from the former $1^{st}-3^{rd}$ and $2^{nd}-4^{th}$ the results of a fresh grouping according to values of $\frac{4}{3}V_m$.

It is true that there is a considerable loss of accuracy, because all angles within 15° are now treated as having the same sine and cosine.

§ 10. *Rules for the Partition of the Observations into Groups.*

It appears from the preceding investigations that it is required to divide up the observations into groups. This may be done, with all necessary accuracy, and with great convenience, by dividing the tides just as they would be divided if every H.W. followed L.W., and *vice versâ*, at the mean interval of $6^{h}\cdot2103$.

Now a quarter-lunar-anomalistic period is $165^{h}\cdot3272$, a quarter-lunar period is $163^{h}\cdot9295$, and semi-lunation is $354^{h}\cdot3670$. Hence, dividing these numbers by $6^{h}\cdot2103$, we find that there are 26·62145 tides in a quarter-anomalistic period, 26·3964 in a quarter period, and 57·0612 in a semi-lunation.

It may be remarked in passing that these results show that the $n+1$ of (10), § 4, is 53·243, and the $n+1$ of (25), § 7, is 52·793.

It is, of course, impossible to have a fractional number of tides, and, therefore, we make a small multiplication table of these numbers, and take the nearest integer in each case. For example, in the case of the semi-lunations, we have—

1.	57·0612	57.	4.	228·2448	228.
2.	114·1224	114.	5.	285·3060	285.
3.	171·1836	171.	6.	342·3672	342.

These have to be divided between H. and L.W. For the sake of convenience, I suppose that we always begin the series with a H.W., then when the integer is odd we put in one more H.W. than L.W., and thus have the following rule:—

No. of semi-lunation ..	1	2	3	4	5	6
No. of last H.W. in the semi-lunation	29	57	86	114	143	171
No. of last L.W. in the semi-lunation	28	57	85	114	142	171

The H.W. and L.W. are here supposed to be numbered consecutively from 1 onwards in separate tables.

The other rules of partition given in Appendix E are found in the same way.

§ 11. *On the Over Tides.*

Observations of H. and L.W. are very inappropriate for the determination of these tides (of which the most important are M_4, M_6, S_4, S_6), because they express the departure of the wave from the simple harmonic shape, and we are supposed to have no information as to what occurs between two tides. These tides make the interval from H. to L.W. longer than from L. to H.W., and there is no doubt that, assuming the existence in the expression for h of a term of the form $A_{2m}\cos 2V_m + B_{2m}\sin 2V_m$, we shall get an approximation to A_{2m} and B_{2m} by finding the mean of $h\cos 2V_m$ and $h\sin 2V_m$. But the computation of the F, G, f, g, coefficients for the perturbation of M_4 by M_2 would be essential, and thus the amount of additional computation would be very great, whereas in the analysis of continuous observation the overtides are found almost without any additional work. I am inclined to think that it would be best to obtain hourly observations for several days at several parts of a lunation, and by some methods of interpolation to construct a typical semi-diurnal tide-wave, from which, by the ordinary methods of harmonic analysis, we could find the ratio of the heights of the over-tides to the fundamental, and the relationship of their phases.

I make no attempt at such an investigation in this place.

§ 12. *On the Annual and Semi-annual Tides.*

These tides are frequently of much importance, so that they ought not to be neglected from a navigational point of view. It is obviously impossible to obtain any results from a series of observations of less than a year's duration.

Rules for the partition of tides into months or 12[th] parts of a year are given in the Appendix E. The mean of all the H. and L.W. observations for each month may be taken as the height of mean water

at the middle of the month, and the 12 values for the year may be submitted to the ordinary processes of harmonic analysis for the evaluation of these two tides.

We have supposed in the previous investigation that the tide heights are measured from mean sea-level, and although it is not necessary that this condition should be rigorously satisfied, it might be well, where there is a large annual tide, to refer the heights to different datum levels in the different quarters of the year.

§ 13. *On Gaps in the Series of Observations.*

It often happens in actual observations that a few tides are missing through some accident, or are obviously vitiated by heavy weather. Now the present method depends for its applicability on the evanescence of terms in the averages. It is true that it is rigorously applicable even for scattered observations, but if applied to such a case all the F, G, f, g coefficients have to be calculated, and, as every tide reacts on every other, the computation would be so extensive as to make the method almost impracticable. Thus, where there is a gap, observations must be fabricated (of course noting that they *are* fabrications) by some sort of interpolation, and even values which are very incorrect are better than none.* If the interpolation is extensive, it might be well to test its correctness in a few places when the reduction is done. If a whole week or fortnight be missing, and if the computer cannot find a plausible method of interpolation, I can only suggest a preliminary reduction from the continuous parts, and the computation of a tide table for the hiatus. Each such case must be treated on its merits, and it is hardly possible to formulate general rules.

APPENDIX.

Tables and Rules of General Applicability.

A. *To find* $\frac{1}{2}V_m$.

The following table is for finding what would be the mean moon's hour-angle, if the moon had been on the meridian at the epoch. This angle is denoted by $\frac{1}{2}V_m$ or $(\gamma-\sigma)t$, and is equal to the angle through which the earth has turned relatively to the mean moon (at $14^\circ{\cdot}4920521$ per mean solar hour) since epoch.

* Fabricated times and heights would very likely be no worse than real observations during a few days of rough weather. A perfect tide table only claims to predict the tide apart from the influence of wind and atmospheric pressure; and, conversely, tidal observations must be sufficiently numerous to eliminate these influences by averages.

It would be advantageous to extend the table up to 90 days, but it can be used as it is for periods greater than 30 days by the division of the time into sets of 30 days. In the second period of 30 days $5^\circ\cdot 7$ must be *subtracted* from the tabular entry, for the third period $11^\circ\cdot 4$, and so on.*

For example: Find $\frac{1}{2}V_m$ for $78\frac{1}{2}^d\ 11^h\ 23^m$. The day is $18\frac{1}{2}$ of the third 30, and the tabular entry for $18\frac{1}{2}^d\ 11^h$ is $113^\circ\cdot 9$, and subtracting $11^\circ\cdot 4$ we have $102^\circ\cdot 5$; 23^m gives $5^\circ\cdot 6$, so that $\frac{1}{2}V_m = 108^\circ\cdot 1$. The correct result is $107^\circ\cdot 99$, and it is obvious that an error of $0^\circ\cdot 1$ may easily be incurred by the use of the table.

The row for day $-\frac{1}{2}$ is given because it may be necessary to use one tide before epoch; this row is used in the example below.

* Observe that the decimals run thus, ·0, ·5, ·0, &c., then ·4, ·9, ·4, &c., then ·8, ·3, ·8, &c., and so on. The first entry in which the sequence alters I call a "change." The incidence of "changes" may be found thus: $\gamma - \sigma$ is $14\frac{1}{2} - \cdot 00795$; take Crelle's multiplication table for 795, and note where the last digit but three, having been 4, becomes 5; I say that this is a "change." For example, $559 \times 795 = 444405$, and $560 \times 795 = 445200$; then a change occurs at the 560^{th} hour, or at the $(12 \times 46 + 8)^{th}$ hour, or at $23^d\ 8^h$. If the table be continued to 60 and 90 days, &c., by subtracting $5^\circ\cdot 7$, $11^\circ\cdot 4$, &c., the changes will fall a little wrong, but they may easily be corrected by means of Crelle's table, as here shown.—(Added Aug. 2, 1890.)

Table of $(\gamma - \sigma)t$ or $\frac{1}{2}V_m$.

Days.	0h.	1h.	2h.	3h.	4h.	5h.	Days.	6h.	7h.	8h.	9h.	10h.	11h.
−½	186°·1	200°·6	215°·1	229°·6	244°·1	258°·6	−½	273°·0	287°·5	302°·0	316°·5	331°·0	345°·5
0	0	14·5	29·0	43·5	58·0	72·5	0	87·0	101·4	115·9	130·4	144·9	159·4
½	173·9	188·4	202·9	217·4	231·9	246·4	½	260·9	275·3	289·8	304·3	318·8	333·3
1	347·8	2·3	16·8	31·3	45·8	60·3	1	74·8	89·3	103·7	118·2	132·7	147·2
½	161·7	176·2	190·7	205·2	219·7	234·2	½	248·7	263·2	277·7	292·1	306·6	321·1
2	335·6	350·1	4·6	19·1	33·6	48·1	2	62·6	77·1	91·6	106·0	120·5	135·0
½	149·5	164·0	178·5	193·0	207·5	222·0	½	236·5	251·0	265·5	280·0	294·4	308·9
3	323·4	337·9	352·4	6·9	21·4	35·9	3	50·4	64·9	79·4	93·9	108·3	122·8
½	137·3	151·8	166·3	180·8	195·3	209·8	½	224·3	238·8	253·3	267·8	282·3	296·7
4	311·2	325·7	340·2	354·7	9·2	23·7	4	38·2	52·7	67·2	81·7	96·2	110·6
½	125·1	139·6	154·1	168·6	183·1	197·6	½	212·1	226·6	241·1	255·6	270·1	284·6
5	299·0	313·5	328·0	342·5	357·0	11·5	5	26·0	40·5	55·0	69·5	84·0	98·5
½	113·0	127·4	141·9	156·4	170·9	185·4	½	199·9	214·4	228·9	243·4	257·9	272·4
6	286·9	301·3	315·8	330·3	344·8	359·3	6	13·8	28·3	42·8	57·3	71·8	86·3
½	100·8	115·3	129·7	144·2	158·7	173·2	½	187·7	202·2	216·7	231·2	245·7	260·2
7	274·7	289·2	303·6	318·1	332·6	347·1	7	1·6	16·1	30·6	45·1	59·6	74·1
½	88·6	103·1	117·6	132·0	146·5	161·0	½	175·5	190·0	204·5	219·0	233·5	248·0
8	262·5	277·0	291·5	305·9	320·4	334·9	8	349·4	3·9	18·4	32·9	47·4	61·9
½	76·4	90·9	105·4	119·9	134·3	148·8	½	163·3	177·8	192·3	206·8	221·3	235·8
9	250·3	264·8	279·3	293·8	308·3	322·7	9	337·2	351·7	6·2	20·7	35·2	49·7
½	64·2	78·7	93·2	107·7	122·2	136·6	½	151·1	165·6	180·1	194·6	209·1	223·6
10	238·1	252·6	267·1	281·6	296·1	310·6	10	325·0	339·5	354·0	8·5	23·0	37·5
½	52·0	66·5	81·0	95·5	110·0	124·5	½	138·9	153·4	167·9	182·4	196·9	211·4
11	225·9	240·4	254·9	269·4	283·9	298·4	11	312·9	327·3	341·8	356·3	10·8	25·3
½	39·8	54·3	68·8	83·3	97·8	112·3	½	126·8	141·3	155·7	170·2	184·7	199·2
12	213·7	228·2	242·7	257·2	271·7	286·2	12	300·7	315·2	329·6	344·1	358·6	13·1
½	27·6	42·1	56·6	71·1	85·6	100·1	½	114·6	129·1	143·6	158·0	172·5	187·0
13	201·5	216·0	230·5	245·0	259·5	274·0	13	288·5	303·0	317·5	331·9	346·4	0·9
½	15·4	29·9	44·4	58·9	73·4	87·9	½	102·4	116·9	131·4	145·9	160·3	174·8
14	189·3	203·8	218·3	232·8	247·3	261·8	14	276·3	290·8	305·3	319·8	334·2	348·7
½	3·2	17·7	32·2	46·7	61·2	75·7	½	90·2	104·7	119·2	133·7	148·2	162·6

mins.	
1	0°·2
2	0·5
3	0·7
4	1·0
5	1·2
6	1·4
7	1·7
8	1·9
9	2·2
10	2·4
11	2·7
12	2·9
13	3·1
14	3·4
15	3·6
16	3·9
17	4·1
18	4·3
19	4·6
20	4·8
21	5·1
22	5·3
23	5·6
24	5·8
25	6·0
26	6·3
27	6·5
28	6·8
29	7·0

15	177·1	191·6	206·1	220·6	235·1	249·6	15	264·1	278·6	293·1	307·6	322·1	336·6	30	7·3
½	351·0	5·5	20·0	34·5	49·0	63·5	½	78·0	92·5	107·0	121·5	136·0	150·5	31	7·5
16	164·9	179·4	193·9	208·4	222·9	237·4	16	251·9	266·4	280·9	295·4	309·9	324·4	32	7·7
½	338·9	353·3	7·8	22·3	36·8	51·3	½	65·8	80·3	94·8	109·3	123·8	138·3	33	8·0
17	152·8	167·2	181·7	196·2	210·7	225·2	17	239·7	254·2	268·7	283·2	297·7	312·2	34	8·2
½	326·7	341·2	355·6	10·1	24·6	39·1	½	53·6	68·1	82·6	97·1	111·6	126·1	35	8·5
18	140·6	155·1	169·5	184·0	198·5	213·0	18	227·5	242·0	256·5	271·0	285·5	300·0	36	8·7
½	314·5	329·0	343·5	357·9	12·4	26·9	½	41·4	55·9	70·4	84·9	99·4	113·9	37	8·9
19	128·4	142·9	157·4	171·9	186·3	200·8	19	215·3	229·8	244·3	258·8	273·3	287·8	38	9·2
½	302·3	316·8	331·3	345·8	0·2	14·7	½	29·2	43·7	58·2	72·7	87·2	101·7	39	9·4
20	116·2	130·7	145·2	159·7	174·2	188·6	20	203·1	217·6	232·1	246·6	261·1	275·6	40	9·7
½	290·1	304·6	319·1	333·6	348·1	2·5	½	17·0	31·5	46·0	60·5	75·0	89·5	41	9·9
21	104·0	118·5	133·0	147·5	162·0	176·5	21	190·9	205·4	219·9	234·4	248·9	263·4	42	10·1
½	277·9	292·4	306·9	321·4	335·9	350·4	½	4·9	19·3	33·8	48·3	62·8	77·3	43	10·4
22	91·8	106·3	120·8	135·3	149·8	164·3	22	178·8	193·2	207·7	222·2	236·7	251·2	44	10·6
½	265·7	280·2	294·7	309·2	323·7	338·2	½	352·7	7·2	21·6	36·1	50·6	65·1	45	10·9
23	79·6	94·1	108·6	123·1	137·6	152·1	23	166·6	181·1	195·6	210·0	224·5	239·0	46	11·1
½	253·5	268·0	282·5	297·0	311·5	326·0	½	340·5	355·0	9·5	23·9	38·4	52·9	47	11·4
24	67·4	81·9	96·4	110·9	125·4	139·9	24	154·4	168·9	183·4	197·8	212·3	226·8	48	11·6
½	241·3	255·8	270·3	284·8	299·3	313·8	½	328·3	342·8	357·3	11·8	26·3	40·7	49	11·8
25	55·2	69·7	84·2	98·7	113·2	127·7	25	142·2	156·7	171·2	185·7	200·2	214·6	50	12·1
½	229·1	243·6	258·1	272·6	287·1	301·6	½	316·1	330·6	345·1	359·6	14·1	28·5	51	12·3
26	43·0	57·5	72·0	86·5	101·0	115·5	26	130·0	144·5	159·0	173·5	188·0	202·5	52	12·6
½	216·9	231·4	245·9	260·4	274·9	289·4	½	303·9	318·4	332·9	347·4	1·9	16·4	53	12·8
27	30·8	45·3	59·8	74·3	88·8	103·3	27	117·8	132·3	146·8	161·3	175·8	190·3	54	13·0
½	204·8	219·2	233·7	248·2	262·7	277·2	½	291·7	306·2	320·7	335·2	349·7	4·2	55	13·3
28	18·7	33·1	47·6	62·1	76·6	91·1	28	105·6	120·1	134·6	149·1	163·6	178·1	56	13·5
½	192·6	207·1	221·5	236·0	250·5	265·0	½	279·5	294·0	308·5	323·0	337·5	352·0	57	13·8
29	6·5	21·0	35·5	49·9	64·4	78·9	29	93·4	107·9	122·4	136·9	151·4	165·9	58	14·0
½	180·4	194·9	209·4	223·8	238·3	252·8	½	267·3	281·8	296·3	310·8	325·3	339·8	59	14·3
30	− 5·7														
60	−11·4														
90	−17·2														
120	−22·9														
150	−28·6														

To find V_s.

No table is necessary for the conversion of time into angle at 30° per hour to find V_s, or $2(\gamma-\eta)t$, since we multiply the hours by 30, and add half the number of minutes. This rule is the same for every day.

B. *The tides* S_2 *and* K_2.

It is required to compute U and ϕ from

$$U\cos\phi = \Pi + \lambda_n f''\cos\varpi,$$

$$U\sin\phi = \quad \lambda_n f''\sin\varpi,$$

where $$\varpi = 2h_0 - 2\nu'' + \alpha_n,$$

and $$\Pi = 1 + 3\left(\frac{\text{sun's parx.} - \text{mean parx.}}{\text{mean parx.}}\right),$$

the sun's parallax referred to being its value at the middle of the period under reduction.

If, for example, February 14 is the middle of the period, Π is found thus:—

Sun's parx. Feb. 14 = 8″·95, mean parx. = 8″·85, diff. = +0″·10.

Then $$\Pi = 1 + \frac{3\times 0\cdot10}{8\cdot85} = 1\cdot034.$$

The period under reduction consists in this case of an exact number of semi-lunations. The following table gives λ_n and α_n, according to the number of semi-lunations:—

No. of semi-lunations.	1.	2.	3.	4.	5.	6.
$\log \lambda_n$	9·4300	9·4159	9·3920	9·3575	9·3113	9·2517
α_n	14°·28	28°·82	43°·36	57°·90	72°·43	86°·97

h_0 is the sun's mean longitude at epoch, found from Naut. Al.; and $2\nu''$, f'' are found from Baird's Manual in the tables applicable to the tide K_2.

C. *The Tides* N *and* L.

Summations are carried out over quarter-lunar-anomalistic periods, numbered i, ii, iii, &c. Grand totals are then made in two different ways, viz.:—

$$[\Sigma(\text{i}+\text{ii})-\Sigma(\text{iii}+\text{iv})]+[\Sigma(\text{v}+\text{vi})-\Sigma(\text{iii}+\text{iv})]$$
$$+[\Sigma(\text{v}+\text{vi})-\Sigma(\text{vii}+\text{viii})]+\&\text{c., to find } S^{\circ},$$

and $$[\Sigma(\text{ii}+\text{iii})-\Sigma(\text{iv}+\text{v})]+[\Sigma(\text{vi}+\text{vii})-\Sigma(\text{iv}+\text{v})]$$
$$+[\Sigma(\text{vi}+\text{vii})-\Sigma(\text{viii}+\text{ix})]+\&\text{c., to find } S^{\frac{1}{2}\pi},$$

where, for example, $\Sigma(\text{i}+\text{ii})$ denotes summation carried over the half period made up of i and ii. These totals are multiplied by certain mean cosines and sines (whose values are given in F), and are summed. The next process is multiplication by a factor Φ $(\pi/4(n+1)(m-1)$ of § 4), of which the value depends on the number of quarter-lunar-anomalistic periods under treatment. The following table gives the value of this factor :—

No. of ¼-lunar-anom. periods.	iii.	v.	vii.	ix.	xi.	xiii.
Φ	0·02950	0·01475	0·00738	0·00492	0·00369	0·00295

The angle j is also required; it depends on the time of the first tide under reduction. If t_0 be the time in hours since epoch to the first tide,

$$j = 1^\circ{\cdot}690 - 0^\circ{\cdot}5444\, t_0.$$

For instance, in the example below the first tide is at $3^{\text{h}}\,14^{\text{m}}$ of day $-\frac{1}{2}$; this is $8^{\text{h}}\,46^{\text{m}}$, or $8^{\text{h}}{\cdot}77$, *before* epoch, so that $t_0 = -8^{\text{h}}{\cdot}77$; then

$$j = 1^\circ{\cdot}690 + 0^\circ{\cdot}5444 \times 8{\cdot}77 = +6^\circ{\cdot}46.$$

D. *The Tides* K_1, O, P.

Summations are carried out over quarter-lunar periods numbered I, II, III, &c., and totals are formed like those mentioned in C, and a factor Ψ (which differs slightly from Φ) is required in the formation of S° and $S^{\frac{1}{2}\pi}$. This factor depends on the number of quarter-lunar periods under treatment, and the following table gives its value :—

No. of ¼-lunar periods.	III.	V.	VII.	IX.	XI.	XIII.
Ψ	0·02976	0·01488	0·00744	0·00496	0·00372	0·00298

The angles i and l are required; they depend on the time of the first tide under reduction. If t_0 be the time in hours of the first tide since epoch,

$$i = 1^\circ{\cdot}705 - 0^\circ{\cdot}549\, t_0.$$

$$l = -0^\circ{\cdot}255 + 0^\circ{\cdot}082\, t_0.$$

For instance, in the example below we have, as shown in C, $t_0 = -8^{\text{h}}{\cdot}77$, and

$$i = +6^\circ{\cdot}52,$$

$$l = -0^\circ{\cdot}97.$$

It is required to compute T and ψ from

$$T \cos \psi = f' - \rho_n \cos \theta,$$

$$T \sin \psi = \rho_n \sin \theta,$$

where $$\theta = 2h_0 - \nu' + l + \beta_n.$$

The period under reduction consists in this case of an exact number of quarter-lunar periods, and the following table gives the values of ρ_n and β_n, according to the number of quarter-lunar periods:—

No. of ¼-lunar periods.	III.	V.	VII.	IX.	XI.	XIII.
$\log \rho_n$	9·5749	9·5628	9·5508	9·5303	9·5009	9·4618
β_n	20°·20	33°·66	47°·13	60°·59	74°·06	87°·52

h_0 is the sun's mean longitude at epoch, the formula for l is given above, and ν', f' are found from Baird's Manual in the tables applicable to the tide K_1.

E. *Rules for the Partition of the Observations into Groups.*

If the first event after epoch is a L.W., either omit it from the reductions, or let the first tide be the H.W. which precedes epoch. Thus we are to begin with a H.W.*

The H.W. and L.W. are treated apart in separate tables.

Each tide (H.W. or L.W., as the case may be) is numbered consecutively, from 1 onwards.

The following are rules for partitions:—

* This is not necessary, but it makes the statements of the subsequent rules simpler, as they have not to be given in an alternative form.

For Tides N and L.

Quarter-lunar-anomalistic periods, numbered i, ii, iii, &c.

No. of ¼-lunar-anomalistic periods	i.	ii.	iii.	iv.	v.	vi.	vii.	viii.	ix.	x.	xi.	xii.	xiii.
No. of last H.W. in the ¼ period	14	27	40	53	67	80	93	107	120	133	147	160	173
No. of last L.W. in the ¼ period	13	26	40	53	66	80	93	106	120	133	146	159	173
Total No. of tides up to end of each ¼ period	27	53	80	106	133	160	186	213	240	266	293	319	346

For Tides K_1, O, P.

Quarter-lunar periods, numbered I, II, III, &c.

No. of ¼-lunar periods	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.
No. of last H.W. in the ¼ period	13	27	40	53	66	79	93	106	119	132	145	159	172
No. of last L.W. in the ¼ period	13	26	39	53	66	79	92	105	119	132	145	158	171
Total No. of tides up to end of each ¼ period	26	53	79	106	132	158	185	211	238	264	290	317	343

For M_2 and S_2.

Semi-lunations numbered 1, 2, 3, &c.

No. of semi-lunation	1	2	3	4	5	6
No. of last H.W. in the semi-lunation	29	57	86	114	143	171
No. of last L.W. in the semi-lunation	28	57	85	114	142	171
Total No. of tides up to end of each semi-lunation	57	114	171	228	285	342

For Annual and Semi-annual Tides.

Months, or $\frac{1}{12}$th parts of a year, numbered 1, 2, 3.

No. of month	1	2	3
No. of last H.W. in month	59	118	176
No. of last L.W. in month	59	117	176
Total No. of tides up to end of each month	118	235	352
No. of tides in each month	118	117	117

The epoch for the second quarter year should be 91 days after first epoch, that for the third 92 days after the first, for the fourth 91 days after the third, except in leap year, when the last should also be 92 days.

There are six tides (or about thirty-seven hours) more in a quarter year than in xiii quarter-lunar-anomalistic periods; the times of these six tides (or ten tides in one of the quarters) are to be omitted from the reduction, and their heights are only required when the annual or semi-annual tides are to be found.

F. *Cosine and Sine Factors for all the Tides.*

These are the cosines and sines of 2° 30′, 7° 30′, 12° 30′, &c. They are as follows:—

Cosine and Sine Factors.

Read downwards for cosines, upwards for sines.

1.	0·999	10.	0·676
2.	0·991*	11.	0·609*
3.	0·976	12.	0·537
4.	0·954	13.	0·462
5.	0·924*	14.	0·383*
6.	0·887	15.	0·301
7.	0·843	16.	0·216
8.	0·793*	17.	0·130*
9.	0·737	18.	0·044

In the evaluation of $S^{\circ}\frac{\cos}{\sin}\frac{3}{2}V_m$ and $S^{\frac{1}{2}\pi}\frac{\cos}{\sin}\frac{3}{2}V_m$, only the factors marked * are required.

G. *Increments of Arguments in Various Times.*

The following table gives the increments of arguments of the several tides in various periods, multiples of 360° being subtracted. This table facilitates verification of the calculation of the harmonic constants.

	M_2.	S_2.	K_2.	N.
1 hour.....	28°·984104	30°·00000	30°·08214	28°·43973
1 day......	− 24 ·3815	0	+ 1 ·9713	− 37 ·4465
10 days.....	+116 ·185	0	+ 19 ·713	− 14 ·465
100 days	+ 81 ·85	0	−162 ·87	−144 ·65
	L.	K_1.	O.	P.
1 hour.....	29°·52848	15°·04107	13°·94304	14°·95893
1 day......	− 11 ·3165	+ 0 ·9856	− 25 ·3671	− 0 ·9856
10 days.....	−113 ·165	+ 9 ·856	+106 ·329	− 9 ·856
100 days	− 51 ·65	+98 ·56	− 16 ·71	− 98 ·56

Example.

(a.) *Place, Time, Datum Level, and Unit of Length.*

The case chosen is three months of observation (in reality the tidal predictions of the Indian Government) at Bombay, and the epoch is 0^h, January 1, 1887.

A datum at or very near mean water-mark is taken, so that all the H.W. are positive and the L.W. negative. This datum is found by taking the mean of all the H.W. and L.W. of the original observations. In this case 99 inches was subtracted from all the tide heights. I might more advantageously have subtracted 102 or 103 inches, but 99 inches was chosen from considerations applicable to my earlier attempts, but which do not apply to the computation in its present form.

At places where there is a large annual inequality in the height of water, it would be advisable to use a different datum for each quarter of a year. It is not, however, important that the datum should conform rigorously to mean water-mark, for even the discrepancy of 3½ inches, which occurs in my example, does not materially affect the result.

In recording the heights, a convenient unit of length is to be used, and it is advantageous that the H.W. and the L.W. should be expressible by two figures, so that the larger H.W. and L.W. shall fall into the eighties and nineties. The unit of length is here the inch.

(b.) *Times and Angles.*

The times of H.W., numbered consecutively, are entered in a table, as shown on p. 313. Since 0^h astronomical time is the epoch, the P.M. tides will come in the half days which are numbered with integrals, and the A.M. tides in the half days which fall between the integral numbers.

From time to time there will be a half day with no H.W.; this row in the table should be left blank, but there happens to be no such row in the sample shown. A computation form for times and angles might be printed, for, although the exigencies of the printer have not allowed the entries to be equally spaced in the sample below, yet the computation form might be printed with equal spaces, and the dividing lines are to be filled in by hand.

The L.W. table is similar.

Both H. and L.W. are to be divided into quarter-lunar-anomalistic and quarter-lunar periods, and semi-lunations, according to the rules given in E. These partitions and the numbering of the entries could not be printed, because of the occasional blank rows.

The formation of $\frac{1}{2}V_m$ and of V_s, by means of Table A and the rule following it, is obvious. In the subtractions and additions under the headings $\frac{1}{2}V_m - V_s$ and $\frac{1}{2}V_m + V_s$, 360° is added or subtracted where necessary. V_m is found by doubling $\frac{1}{2}V_m$.

(c.) *The Heights.*

The H.W. heights are written in columns (with the same blanks as in the table of times and angles), and are so arranged, either on strips of paper, or by folding the paper, that the heights may be pinned to the times, bringing each height opposite to an angle on the same row with the time corresponding to that height. The heights will on one occasion have to be pinned opposite the V_m column, on a second occasion opposite the V_s column, and on a third occasion opposite the $\frac{1}{2}V_m$ column.

The L.W. heights are written in similar columns, but the minus signs should be omitted.

It is well to divide the columns, or to put fiducial marks in the table for easy verification of the proper allocation of the heights with the times. Any marks suffice, but the division into quarter-anomalistic periods, as shown below, seems to be as good as any other.

If it is proposed to evaluate the annual and semi-annual tides, it is necessary to carry on the heights beyond the times by 3 (or 5) H.W. and 3 (or 5) L.W., and to partition them into months. The mean for each month is evaluated, and if the successive quarters of the year are referred to different data, the mean monthly heights must all be referred to a common datum. This process is not carried out

H.W.

No. of entry.	Day.	Time.	min.	hr.	$\frac{1}{2}V_m$.	min.	hr.	V_s.	$\frac{1}{2}V_m - V_s$.	$\frac{1}{2}V_m + V_s$.	V_m.	$\frac{1}{2}$-Anom. periods.	$\frac{1}{2}$-Lunar periods.	Semi-lunai ons.
		h. m.												
1	$-\frac{1}{2}$	3 14	3·4	229·6	233·0	7·0	90	97·0	136·0	330·0	106·0	i begins	I begins	1 begins
2	0	3 38	9·2	43·5	52·7	19·0	90	109·0	303·7	161·7	105·4			
3	$\frac{1}{2}$	3 49	11·8	217·4	229·2	24·5	90	114·5	114·7	343·7	98·4			
4	1	4 31	7·5	45·8	53·3	15·5	120	135·5	277·8	188·8	106·6			
5	$\frac{1}{2}$	4 30	7·2	219·7	226·9	15·0	120	135·0	91·9	1·9	98·8			
6	2	5 46	11·1	48·1	59·2	23·0	150	173·0	246·2	232·2	118·4			
7	$\frac{1}{2}$	5 20	4·8	222·0	226·8	10·0	150	160·0	66·8	26·8	93·6			
8	3	7 15	3·6	64·9	68·5	7·5	210	217·5	211·0	286·0	137·0			
9	$\frac{1}{2}$	6 29	7·0	224·3	231·3	14·5	180	194·5	36·8	65·8	102·6			
10	4	8 28	6·8	67·2	74·0	14·0	240	254·0	180·0	328·0	148·0			
11	$\frac{1}{2}$	7 40	9·7	226·6	236·3	20·0	210	230·0	6·3	106·3	112·6			
12	5	9 22	5·3	69·5	74·8	11·0	270	281·0	153·8	355·8	149·6			
13	$\frac{1}{2}$	8 47	11·4	228·9	240·3	23·5	240	263·5	36·8	143·8	120·6		I ends	
14	6	10 9	2·2	71·8	74·0	4·5	300	304·5	129·5	18·5	148·0	i ends	II begins	
15	$\frac{1}{2}$	9 43	10·4	231·2	241·6	21·5	270	291·5	310·1	173·1	123·2	ii begins		
	&c.			&c.			&c.		&c.	&c.	&c.		&c.	
169	$\frac{1}{2}$	1 22	5·3	220·0	225·3	11·0	30	41·0	184·3	266·3	90·6			
170	87	2 7	1·7	48·4	50·1	3·5	60	63·5	346·6	113·6	100·2			
171	$\frac{1}{2}$	1 53	12·8	207·9	220·7	26·5	30	56·5	164·2	277·2	81·4			6 ends
172	88	2 49	11·8	36·2	48·0	24·5	60	84·5	323·5	132·5	96·0		XIII ends	
173	$\frac{1}{2}$	2 30	7·2	210·1	217·3						74·6	xiii ends		

in my example, because it is useless to attempt the evaluation of these tides of long period from three months of observation.

H.W.
55 i
23
47
19
41
19
37
26
35
37
37
52
42
66 i
50 ii
80
57
91
63
None
99
67
101
66
99
65
92
58 ii
81 iii
&c., &c.

The sum of all the H.W. entries to the end of xiii is 9791. The sum of all the L.W. entries to the end of xiii is 8577. There are 173 H.W. and 173 L.W. Hence mean sea-level is $\frac{1}{346}$ (9791—8577), which is equal to $+3{\cdot}51$. It would have been better, therefore, to have subtracted 103 inches instead of 99 inches from all the heights. This would have given mean sea level at $-0{\cdot}49$ from the datum adopted.

(d.) *Sorting the Heights according to the Values of the Angles.*

In the tables below the column 0° belongs to all angles between 0° and 5°; 5° belongs to 5° to 10°; and so on.

Where an angle falls *exactly* on a multiple of 5°, an arbitrary rule of classification is required, and it is easiest to deem it to belong to the next succeeding 5°, rather than to the preceding 5°.

(e.) *Sorting according to Values of V_m.*

The H. and L.W. are treated in separate tables, similar in form save that the — signs of the L.W. heights are omitted.

The sheets of heights (c) are pinned opposite to the V_m's on the tables of angles (b), and the heights are then entered successively into the columns corresponding to their V_m's in a table like the following.

The division into quarter-lunar-anomalistic periods is maintained, but as this sorting is to serve a double purpose, it is necessary to mark the end of the last semi-lunation. In these tables there are two H.W. and two L.W., which fall after the end of 6 semi-lunations, and before the end of xiii quarter-lunar-anomalistic periods.

Nearly all the entries fall into one quadrant for H.W., and into another for L.W. Thus there are no H.W. entries in the 4th quadrant, and no L.W. entries in 2nd quadrant; there are altogether only 10 H.W. entries in 1st quadrant, and 3 in 3rd quadrant; and there are only 4 L.W. entries in the 1st quadrant, and 17 in 3rd quadrant. Something like this would hold true at all ports.

H.W. V_m.

		Angle	0	5	10	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	No. of entries.
1st quad.	v		.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	39	45	53	4
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	84	
	ix		.	.	.	.	.	.	.	.	.	.	.	.	.	.	38	45	53	.	3
	x		.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	81	.	.	1
	xiii		.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	50	.	1
		6 semi-lunations end																			
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	42	.	.	.	1
2nd quad.		Angle	90	95	100	105	110	115	120	125	130	135	140	145	150	155	160	165	170	175	
	i		41	47	85	55	87	19	42	.	.	26	.	37	.	.	.	.	.	.	14
			87	.	.	23	.	.	.	.	.	.	.	52	.	.	.	.	.	.	
			.	.	.	19	.	.	.	.	.	.	.	66	.	.	.	.	.	.	
	ii		.	.	.	.	.	66	50	99	99	91	80	.	.	.	.	.	.	.	13
			.	.	.	.	.	65	57	.	101	.	.	.	.	.	.	.	.	.	
			.	.	.	.	.	58	63	.	.	.	.	.	.	.	.	.	.	.	
			.	.	.	.	.	.	67	.	.	.	.	.	.	.	.	.	.	.	
			.	.	.	.	.	.	92	.	.	.	.	.	.	.	.	.	.	.	
	iii &c.			&c.					&c.				&c.					&c.			&c.

	xi	.	.	.	45	61	76	96	85	99	.	.	.	.	89	.	.	.	.	14
		.	.	.	.	30	.	88	79	87	.	.	.	.	.	.	.	.	.	
		.	.	.	.	.	.	.	69	45	.	.	.	.	.	.	.	.	.	
		.	.	.	.	.	.	.	57	.	.	.	.	.	.	.	.	.	.	
	xii	.	.	.	.	.	.	.	.	20	.	.	56	66	51	20	44	27	43	11
		.	.	.	.	.	.	.	.	.	.	.	.	.	.	64	.	86	.	
		.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	61	.	
	xiii	67	.	61	59	.	65	.	67	.	60	67	.	.	.	.	.	.	.	11
		.	.	55	.	.	62	.	63	.	.	.	.	.	.	.	.	.	.	
		6 semi-lunations end																		
		.	49	.	.	.	.	.	.	.				.	.	.	.	.	.	
3rd quad.	Angle	180	185	190	195	200	205	210	215	220	225	230	235	240	245	250	255	260	265	
	viii	56																		1
	xii	49	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2
		55	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	
		6 semi-lunations end																		
4th quad.		Nil																	Total	173

L.W. V_A.

																				No. of entries.
1st quad.		Angle	0	5	10	15	20	25	30	35	40	45	50	55	60	[redacted]	70	75	80	85	
	viii		30																		1
	xii		36	19	28																3
			......6 semi-lunations end......																		
2nd quad.			Nil.																		
3rd quad.		Angle	180	185	190	195	200	205	210	215	220	225	230	235	240	245	250	255	260	265	
	i		.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	26	16	.	2
	v		.	.	.	.	.	.	.	.	.	.	.	.	.	22	32	.	41	.	4
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	12	.	.	.	
	ix		.	.	.	.	.	.	.	.	.	.	.	.	.	26	35	48	51	44	7
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	15	.	.	45	
	xiii		.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	36	.	.	4
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	62	.	.	
			......6 semi-lunations end......																		
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	27	.	.	
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	56	.	.	
4th quad.		Angle	270	275	280	285	290	295	300	305	310	315	320	325	330	335	340	345	350	355	
	&c.			&c.					&c.					&c.				&c.			&c.
	xii		.	.	.	.	.	.	.	.	.	.	.	55	.	42	50	13	.	44	10
			.	.	.	.	.	.	.	.	.	.	.	53	.	58	.	44	.	.	
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	47	.	.	
			.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	50	.	.	
	xiii		67	60	.	66	54	.	55	.	60	56	.	.	.	.	.	.	.	.	8
			.	.	.	.	.	.	63	.	.	.	.	.	.	.	.	.	.	.	
			......6 semi-lunations end......																	Total	178

(f.) *Table of Sums for* N *and* L. (See p. 320.)

Maintaining the divisions i, ii, iii, &c., it is now necessary to sum each of the four times 18 vertical columns in each of the xiii divisions, to subtract the 18 columns of the 3rd quadrant from the 18 columns of the 1st, and to subtract the 18 columns of the 4th quadrant from the 18 of the 2nd. The 2nd—4th columns have then to be reversed.

Since in this case nearly all the H.W. fall in the 2nd quadrant, and nearly all the L.W. fall in the 3rd quadrant, it is easy to write down at once 2nd—4th, and 1st—3rd, as shown on the next page.

In this the — signs of the L.W. entries have to be reintroduced, but as the L.W. lie mostly in 3rd quadrant, which enters with negative sign, they become positive again. It thus happens that nearly all the columns come out +; there are, however, a few — in xii.

(g.) *Table of Sums for* M_2. (See p. 321.)

We now disregard the sub-divisions i, ii, iii, &c., and sum the 4 times 18 columns into grand totals, stopping the summations, however, at the end of 6 semi-lunations (*i.e.*, at 171 H.W. and 171 L.W.).

It would hardly be wise to attempt in this case the subtractions 1st—3rd, 2nd—4th, without the intermediate steps.

The following table (p. 321) gives the results.

(h.) *General Rule for Cosine and Sine Summations.*

For 'cosines' the 18 numbers required are derived from (1st—3rd)—(2nd—4th, reversed).

For 'sines' the 18 numbers required are derived from (1st—3rd) + (2nd—4th, reversed).

(i.) *Evaluations of* N *and* L (continued). (See p. 322.)

Cosines.—From Table (f) of Sums enter the 18 'cosine' numbers, in accordance with (h) in xiii vertical columns, and perform the operations indicated in the example on page 322.

The column of 'cosine factors' are those given in F, and Φ is given in C for xiii $\frac{1}{2}$-lunar-anomalistic periods.

(f.) Sums for N *and* L.

	($2^{nd}-4^{th}$) H.W.	78	47	35	97	37	19	42	.	.	26	.	155	.	.	.	.	.	.
	($2^{nd}-4^{th}$) L.W.	9	.	.	.	62	40	216	11	18	.	.	.	.	.	.	.	.	.
	($2^{nd}-4^{th}$) Total	87	47	35	97	99	59	258	11	18	26	.	155	.	.	.	.	.	.
	($1^{st}-3^{rd}$) H.W.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	($1^{st}-3^{rd}$) L.W.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	26	16	.
	($1^{st}-3^{rd}$) Total	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	26	16	.
	($2^{nd}-4^{th}$) Reversed	.	.	.	.	.	.	155	.	26	18	11	258	59	99	97	35	47	87
	&c.	&c.					&c.					&c.					&c.		
xii	$2^{nd}-4^{th}$ H.W.	.	.	.	.	.	.	.	.	20	.	.	56	66	51	84	44	124	42
	$2^{nd}-4^{th}$ L.W.	.	.	.	.	.	.	.	.	.	.	.	110	.	95	50	154	.	44
	($2^{nd}-4^{th}$) Total	.	.	.	.	.	.	.	.	20	.	.	166	66	146	134	198	124	86
	$1^{st}-3^{rd}$ H.W.	−104	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	$1^{st}-3^{rd}$ L.W.	−36	−19	−28	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	($1^{st}-3^{rd}$) Total	−140	−19	−28	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	($2^{nd}-4^{th}$) Reversed	86	124	198	134	146	66	166	.	.	20	.	.	.	.	.	.	.	.
xiii	$2^{nd}-4^{th}$ H.W.	57	49	116	59	.	127	.	129	.	60	67	.	.	.	.	.	.	.
	$2^{nd}-4^{th}$ L.W.	67	50	.	66	54	.	118	.	60	56	.	.	.	.	.	.	.	.
	($2^{nd}-4^{th}$) Total	124	99	116	125	54	127	118	129	60	116	67	.	.	.	.	.	.	.
	$1^{st}-3^{rd}$ H.W.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	42	.	50	.
	$1^{st}-3^{rd}$ L.W.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	181	.	.
	($1^{st}-3^{rd}$) Total	..	..	..	..	..	..	..	..	..	..	..	..	..	..	42	181	50	..
	($2^{nd}-4^{th}$) Reversed	..	..	..	..	..	..	..	67	116	60	129	118	127	54	125	116	99	124

(g.) *Sums for* M_2.

1st H.W. ...	.	.	.	.	.	.	.	.	.	.	.	.	.	.	38	115	148	87
1st L.W. ...	−66	−19	−28	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1st Total	−66	−19	−28	.	.	.	.	.	.	.	.	.	.	.	38	115	148	87
2nd H.W.	262	247	210	452	292	976	1056	1120	941	678	598	696	439	135	395	122	317	216
2nd L.W.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2nd Total ...	262	247	210	452	292	976	1056	1120	941	678	598	696	439	135	395	122	317	216
3rd H.W.	160	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3rd L W. ...	.	.	.	.	.	.	.	.	.	.	.	.	.	−48	−94	−172	−108	−199
3rd Total ...	160	.	.	.	.	.	.	.	.	.	.	.	.	−48	−94	−172	−108	−199
4th H.W.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4th L.W. ...	−264	−188	−54	−481	−548	−810	−1453	−1095	−532	−518	−436	−240	−353	−220	−209	−241	−52	−66
4th Total ...	−264	−188	−54	−481	−548	−810	−1453	−1095	−532	−518	−436	−240	−353	−220	−209	−241	−52	−66
(2nd−4th) Total....	526	435	264	933	840	1786	2509	2215	1478	1196	1034	936	792	355	604	363	369	282
(1st−3rd) Total	−226	−19	−28	.	.	.	.	.	.	.	.	.	.	48	132	287	256	286
(2nd−4th) Reversed.	282	369	363	604	355	792	936	1034	1196	1473	2215	2509	1786	840	933	264	435	526

Cosines.					Sums in pairs.				Differences.					Sum of 5 preceding cols.	× Cosine factors.		
i	ii	&c.	xii	xiii	i + ii	iii + iv	&c.	xi + xii	i + ii − (iii + iv)	v + vi − (iii + iv)	v + vi − (vii + viii)	ix + x − (vii + viii)	ix + x − (xi + xii)			+	−
.	.		−226	.	.	− 62		−226	+ 62	+ 62	+220	+220	+226	+ 790 ×	·999	= 790	.
.	.		−143	.	.	−123		−143	+123	+123	+122	+122	+143	+ 633	·991	628	.
.	.		−226	.	.	−165	&c.	−226	+165	+165	.	.	+226	+ 556	·976	542	.
.	.	&c.	−134	.	.	−182		−134	+182	+182	+288	+288	+134	+1074	·954	1025	.
.	.		−146	.	.	.		−185	.	.	+170	+170	+185	+ 525	·924	485	.
.	.		− 66	.	.	−410		− 66	+410	+410	+316	+316	+ 66	+1518	·887	1346	.
−155	.		−166	.	−155	−182	&c.	−166	+ 27	− 11	−139	−132	− 20	− 275	·843	.	232
.	− 80	&c.	.	− 67	− 80	−334		− 15	+254	+207	+114	+ 71	−155	+ 491	·793	389	.
− 26	− 91		.	−116	−117	−178		− 46	+ 61	− 39	−106	−300	−365	− 749	·737	.	551
− 18	−311		− 20	− 60	−329	−235	&c.	−336	− 94	+ 55	+ 11	+ 49	+194	+ 215	·676	145	.
− 11	−211		.	−129	−222	−166		−378	− 56	−567	−654	−429	−130	−1836	·609	.	1118
−258	−529	&c.	.	−118	−787	−225		−479	−562	− 27	+310	+476	+393	+ 590	·537	317	.
− 59	−371		.	−127	−430	−394		−350	− 36	+179	− 68	+ 24	+227	+ 326	·462	151	.
− 99	− 93		.	− 54	−192	.		− 91	−192	− 69	+233	+218	+ 7	+ 197	·383	75	.
− 97	−163		.	− 83	−260	−163	&c.	− 45	− 97	+ 58	−105	−103	− 58	− 305	·301	.	92
− 9	.	&c.	.	+ 65	− 9	.		.	− 9	− 20	− 20	+ 70	+ 70	+ 91	·216	20	.
− 31	.		.	− 49	− 31	.		.	− 31	− 94	− 94	− 54	− 54	− 327	·130	.	43
− 87	.		.	−124	− 87	.		.	− 87	− 70	− 70	− 69	− 69	− 365	·044	.	16
																+5913	−2052
																−2052	
														Total.....		+3861	
														factor Φ...	×	·00295	
														$S^{\circ}h \cos V_m$ =		+11·38	

Cosines (continued).—Form columns ii + iii, iv + v, vi + vii, viii + ix, x + xi, xii + xiii. Form difference columns (ii + iii) − (iv + v), (vi + vii) − (iv + v), (vi + vii) − (viii + ix), (x + xi) − (viii + ix), (x + xi) − (xii + xii); add the 5 difference columns together; multiply by cosine factors; sum and multiply by Φ or 0·00295.

The result is :—

$$S^{\frac{1}{2}\pi}h \cos V_m = -8{\cdot}38.$$

Sines.—From Table (f) of Sums, enter 18 "sine" numbers, in accordance with (h) in xiii vertical columns.

Perform all the same operations as those on "cosine" numbers, save that we use sine factors, which are the same as cosine factors in inverse order, viz., beginning with 0·044 and ending with 0·999.

The two results are—

$$S^{\circ}h \sin V_m = +5{\cdot}94, \qquad S^{\frac{1}{2}\pi}h \sin V_m = +10{\cdot}06.$$

Collecting results, proceed thus :—

$S^{\circ}h \cos V_m$	$= +11{\cdot}38.$	$S^{\frac{1}{2}\pi}h \cos V_m$	$= -\ 8{\cdot}38.$
$S^{\frac{1}{2}\pi}h \sin V_m$	$= +10{\cdot}06.$	$S^{\circ}h \sin V_m$	$= +\ 5{\cdot}94.$
Sum	$= +21{\cdot}44.$	Sum	$= -\ 2{\cdot}44.$
Diff.	$= +\ 1{\cdot}32.$	Diff.	$= -14{\cdot}32.$
$P = \frac{1}{2}$ sum	$= +10{\cdot}72.$	$R = \frac{1}{2}$ sum	$= -\ 1{\cdot}22.$
$Q = \frac{1}{2}$ diff.	$= +\ 0{\cdot}66.$	$S = \frac{1}{2}$ diff.	$= -\ 7{\cdot}16.$

(i.) *Evaluation* M_2 (continued).

From the Table (g) of Sums for M_2 enter in one vertical column 18 cosine numbers, in accordance with (h); multiply them by cosine factors; add up and divide by the total number of entries for 6 semi-lunations, viz., 342.

The result is :—

$$A_m = \tfrac{1}{342}\Sigma h \cos V_m = -30{\cdot}58.$$

Then enter in vertical column 18 sine numbers, in accordance with (h); multiply them by sine factors, add up, and divide by 342.

The result is :—

$$B_m = \tfrac{1}{342}\Sigma h \sin V_m = +38{\cdot}47.$$

(k.) *Sorting according to Values of* V_s, *and Evaluation of* S_2, K_2.

The H. and L.W. are treated in separate tables, similar in form save that the − signs of the L.W. heights are omitted.

The sheets of heights (c) are pinned opposite to the V_s's on the Tables of Angles (b), and the heights are entered successively into the columns corresponding to their V_s's in a table like (e), which was used for sorting according to values of V_m. The sorting is carried as far as the end of an exact multiple of a semi-lunation,—in this case to the end of 6 semi-lunations. No sub-division is *necessary*, but for the purpose of verification it is useful to break the entries into groups of about 40. This is conveniently done by a division after each third $\frac{1}{4}$-lunar-anomalistic period, so that i, ii, iii would be the first group; iv, v, vi the second; vii, viii, ix the third; and x, xi, xii, and all but the end of xiii, the last.

In this case the entries fall into all the four quadrants with about equal frequency.

We next sum the four times 18 columns, just as with M_2 in (g), and form 1st—3rd and 2nd—4th, reversed, in the same way.

Next we write the 18 cosine numbers, (1st—3rd)—(2nd—4th, reversed) in vertical column, multiply by cosine factors, add, and divide by the total number of entries, which is 342. Afterwards write the sine-numbers (1st—3rd)+(2nd—4th, reversed), multiply by sine factors, add, and divide by 342.

The results are:—

$$A_s = \tfrac{1}{342}\Sigma h \cos V_s = +21{\cdot}08. \qquad B_s = \tfrac{1}{342}\Sigma h \sin V_s = +3{\cdot}62.$$

(l.) *Sorting according to Values of $\frac{1}{2}V_m$.*

The whole process is precisely parallel to the sorting according to values of V_m in (e); the thirteen divisions are, however, given by the quarter-lunar-periods I, II, ... XIII. The only difference lies in the substitution of the factor Ψ (for XIII equal to $0{\cdot}00298$) for Φ. It is unnecessary to give an example.

The results are:—

$$S^{\circ}h \cos \tfrac{1}{2}V_m = -10{\cdot}50, \qquad S^{\circ}h \sin \tfrac{1}{2}V_m = +8{\cdot}04,$$
$$S^{\frac{1}{2}\pi}h \cos \tfrac{1}{2}V_m = +\ 0{\cdot}40, \qquad S^{\frac{1}{2}\pi}h \sin \tfrac{1}{2}V_m = +3{\cdot}74.$$

(m.) *Sorting of $\frac{1}{2}V_m$.*

It is required to find what the sums in (l) would be if every H.W. height had been unity, and every L.W. the same both in magnitude *and sign;* in fact to find $S^{\circ} \cos \frac{1}{2}V_m$, $S^{\frac{1}{2}\pi} \cos \frac{1}{2}V_m$, &c.

This is done by counting the entries in the preceding sorting in (l) without regard to magnitude, taking the L.W. entries as actually positive, instead of being (as they are) negative quantities with the negative sign suppressed.

Since in this case we have simply to count entries which are all treated as positive, the table of sums of H. and L.W. may be written together. The following example gives part of the work—

H. and L.W. $\frac{1}{2}V_m$.

I.																		
2nd	.	.	.	.	.	.	.	1	1	1	.	1	1	1	.	.	.	.
4th	.	.	.	.	.	.	.	.	.	.	.	3	4	.	.	.	.	.
2nd—4th......	.	.	.	.	.	.	.	+1	+1	+1	.	−2	−3	+1	.	.	.	.
1st	.	.	.	.	.	.	.	.	.	.	2	1	.	1	2	.	.	.
3rd	.	.	.	.	.	.	.	.	.	3	2	1	1	.	.	.	.	.
1st—3rd	.	.	.	.	.	.	.	.	.	−3	0	0	−1	+1	+2	.	.	.
2nd—4th, rev.	.	.	.	.	+1	−3	−2	.	+1	+1	+1	.	.	.	.	.	.	.
II.																		
2nd	.	.	.	.	.	.	.	.	.	.	.	.	4	3	.	.	.	.
4th	.	.	.	.	.	.	.	.	.	.	2	3	1	.	.	.	.	.
2nd—4th......	.	.	.	.	.	.	.	.	.	.	−2	−3	+3	+3	.	.	.	.
1st	.	.	.	.	.	.	.	.	.	.	.	.	2	3	2	.	.	.
3rd	.	.	.	.	.	.	.	.	.	.	.	3	4	.	.	.	.	.
1st—3rd	.	.	.	.	.	.	.	.	.	.	.	−3	−2	+3	+2	.	.	.
2nd—4th, rev.	.	.	.	.	+3	+3	−3	−2	.	.	.	.	.	.	.	.	.	.
III.			&c.							&c.						&c.		

We next proceed to form XIII columns of cosine numbers, and generally to operate exactly as though these numbers were heights; and then proceed with XIII columns of sine numbers in the same way.

The results are

$$S^{\circ}\cos\tfrac{1}{2}V_m = -0{\cdot}0522, \qquad S^{\circ}\sin\tfrac{1}{2}V_m = +0{\cdot}0117,$$

$$S^{\frac{1}{2}\pi}\cos\tfrac{1}{2}V_m = -0{\cdot}0168, \qquad S^{\frac{1}{2}\pi}\sin\tfrac{1}{2}V_m = -0{\cdot}0129.$$

(n.) *Formation of the Mean Sums of* cos $\tfrac{3}{2}V_m$ *and* sin $\tfrac{3}{2}V_m$.

These may be found with sufficient accuracy from the last Table (m) of Sums, part of which is given. In that table lines are drawn dividing the columns into three divisions of six each. These are treated in the way shown in the following example:—

H. and L.W. $\tfrac{3}{2}V_m$.

							Refer to preceding sorting (m).
I.	.	.	.	.	.	.	-1^{st} six of $2^{nd}-4^{th}$.
	.	.	.	−3	0	0	$+2^{nd}$ six of $1^{st}-3^{rd}$.
	−3	+1	.	.	.	.	$+3^{rd}$ six of $2^{nd}-4^{th}$.
$2^{nd}-4^{th}$	−3	+1	.	−3	0	0	
	.	.	.	.	.	.	$+1^{st}$ six of $1^{st}-3^{rd}$.
	.	+1	+1	+1	.	−2	$+2^{nd}$ six of $2^{nd}-4^{th}$.
	+1	−1	−2	.	.	.	-3^{rd} six of $1^{st}-3^{rd}$.
$1^{st}-3^{rd}$	+1	0	−1	+1	.	−2	
$2^{nd}-4^{th}$, rev.	0	0	−3	.	+1	−3	
II.	.	.	.	.	.	.	-1^{st} six of $2^{nd}-4^{th}$.
	.	.	.	.	.	−3	$+2^{nd}$ six of $1^{st}-3^{rd}$.
	+3	+3	.	.	.	.	$+3^{rd}$ six of $2^{nd}-4^{th}$.
$2^{nd}-4^{th}$	+3	+3	.	.	.	−3	
	.	.	.	.	.	.	$+1^{st}$ six of $1^{st}-3^{rd}$.
	.	.	.	.	−2	−3	$+2^{nd}$ six of $2^{nd}-4^{th}$.
	+2	−3	−2	.	.	.	-3^{rd} six of $1^{st}-3^{rd}$.
$1^{st}-3^{rd}$	+2	−3	−2	.	−2	−3	
$2^{nd}-5^{th}$, rev.	−3	.	.	.	+3	+3	
III.			&c.				&c.

We have now only 6 instead of 18 sub-divisions of the quadrant, but the cosine and sine numbers are found in exactly the same way as before.

The following example shows part of the treatment, and the cosine factors are those marked * in F.

Cosines.				Sums in pairs.				Differences.			Sum of 5 columns.		Cosine factors.	+	−
I.	II.		XIII.	I + II	III + IV.		XI + XII.	(I + II). −(III + IV).		(IX + X). −(XI + XII).					
+1	+5	&c.	0	+6	−2	&c.	+3	+8	&c.	0	+23	×	0·991	22·973	.
0	−3		−1	−3	+3		−1	−6		+1	− 8		0·924	.	7·392
+2	−2		0	0	0		−2	0		−4	−16		0·793	.	12·688
+1	.		+1	+1	−5	&c.	+1	+6	&c.	−1	+20		0·609	12·180	.
−1	−5	&c.	−1	−6	+2		−3	−8		+1	−17		0·383	.	6·511
+1	−6		+1	−5	−1		−3	−4		+7	+20		0·130	3·120	.
														38·273	26·591
														26·591	
														+11·682	
											Factor Ψ ...			×0·00298	
											$S^{\circ} \cos \frac{3}{2} V_m =$			+0·0347	

The remaining process is exactly like that pursued before, and the four results are

$$S^\circ \cos \tfrac{3}{2}V_m = +0{\cdot}0347, \qquad S^\circ \sin \tfrac{3}{2}V_m = -0{\cdot}1830,$$

$$S^{\frac{1}{2}\pi} \cos \tfrac{3}{2}V_m = -0{\cdot}0479, \qquad S^{\frac{1}{2}\pi} \sin \tfrac{3}{2}V_m = -0{\cdot}0173.$$

(o.) *The Sorting of $\frac{1}{2}V_m + V_s$ and of $\frac{1}{2}V_m - V_s$.*

These angles have to be sorted without reference to the heights, or just as though all the heights were unity. Every entry is to be regarded as unity. The following example shows part of the sorting of $\frac{1}{2}V_m + V_s$, and 1 denotes a H.W., † a L.W.; by this device H. and L.W. may be sorted on the same paper.

We may also, if it is found convenient, put on it the sorting of $\frac{1}{2}V_m - V_s$ by adopting, say 0, to denote a H.W. and * a L.W., each one of these four signs denoting simply unity.

H. and L.W. $\frac{1}{2}V_m + V_s$. 1 for H.W., † for L.W.

1st quad.	Angles	0	5	10	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85
I.		1	.	†	.	.	1	.	†	†	.	.	.	1	1†	.	.	.	.
II.		†	.	.	1	†	.	.	1	.	†	.	1	.	.	1	.	.	†
				&c.						&c.						&c.			

2nd quad.	Angles	90	95	100	105	110	115	120	125	130	135	140	145	150	155	160	165	170	175
I.		.	.	.	1†	.	.	.	.	.	.	1	.	.	.	1	†	.	.
II.		1	.	.	†	1	.	.	1†	.	.	.	†	.	.	.	†	1	.
				&c.						&c.						&c.			

3rd quad.	Angles 180, &c.
	&c.

4th quad.	Angles 270, &c.
	&c.

We then proceed to count these 1's and †'s just as was done with the number of entries in the sorting of $\frac{1}{2}V_m$, and to operate on them in the same way.

The results are

$$S^0 \cos(\tfrac{1}{2}V_m - V_s) = -\cdot 0078, \qquad S^0 \sin(\tfrac{1}{2}V_m - V_s) = -\cdot 0060,$$
$$S^{\frac{1}{2}\pi} \cos(\tfrac{1}{2}V_m - V_s) = +\cdot 0280, \qquad S^{\frac{1}{2}\pi} \sin(\tfrac{1}{2}V_m - V_s) = +\cdot 0078,$$
$$S^0 \cos(\tfrac{1}{2}V_m + V_s) = +\cdot 1244, \qquad S^0 \sin(\tfrac{1}{2}V_m + V_s) = +\cdot 0094,$$
$$S^{\frac{1}{2}\pi} \cos(\tfrac{1}{2}V_m + V_s) = +\cdot 0147, \qquad S^{\frac{1}{2}\pi} \sin(\tfrac{1}{2}V_m + V_s) = +\cdot 0834.$$

(p).—*Evaluation of* $F_m^{(o)}$, $G_m^{(o)}$, $f_m^{(o)}$, $g_m^{(o)}$, $F_m^{(\frac{1}{2}\pi)}$, $G_m^{(\frac{1}{2}\pi)}$, $f_m^{(\frac{1}{2}\pi)}$, $g_m^{(\frac{1}{2}\pi)}$, $F_s^{(o)}$, $G_s^{(o)}$, $f_s^{(o)}$, $g_s^{(o)}$, $F_s^{(\frac{1}{2}\pi)}$, $G_s^{(\frac{1}{2}\pi)}$, $f_s^{(\frac{1}{2}\pi)}$, $g_s^{(\frac{1}{2}\pi)}$.

These 16 coefficients are required to correct the four sums $S^0 h \begin{smallmatrix}\cos\\ \sin\end{smallmatrix} \frac{1}{2}V_m$, $S^{\frac{1}{2}\pi} h \begin{smallmatrix}\cos\\ \sin\end{smallmatrix} \frac{1}{2}V_m$, for the influence of the tides M_2 and S_2.

I call $S^0 h \cos \frac{1}{2}V_m$ + corrn., W, $S^0 h \sin \frac{1}{2}V_m$ + corrn., X, and the other two Y and Z.

The correction to be applied to $S^0 h \cos \frac{1}{2}V_m$ to get W is

$$-[F_m^{(o)}A_m + G_m^{(o)}B_m + F_s^{(o)}A_s + G_s^{(o)}B_s],$$

and the correction to be applied to $S^0 h \sin \frac{1}{2}V_m$ to get X is

$$-[f_m^{(o)}A_m + g_m^{(o)}B_m + f_s^{(o)}A_s + g_s^{(o)}B_s],$$

and the two other corrections are given by symmetrical formulæ with $(\frac{1}{2}\pi)$ in place of (o).

These coefficients are computed from S^0 and $S^{\frac{1}{2}\pi}$ of $\begin{smallmatrix}\cos\\ \sin\end{smallmatrix} (\frac{1}{2}V_m \pm V_m)$ and of $\begin{smallmatrix}\cos\\ \sin\end{smallmatrix} (\frac{1}{2}V_m \pm V_s)$, as given in (m) (n) (o). It must be especially noticed that we have above in (m) computed S^0 and S of $\sin \frac{1}{2}V_m$; but $\frac{1}{2}V_m - V_m = -\frac{1}{2}V_m$, so that the signs of our previous results must be changed in these two cases.

If we remark that k_m and k_s are constants found by theoretical considerations, that A_m, B_m, A_s, B_s, are already found, and that in the first column we are compelled to omit the affixes to the letters S, k, and the F's and G's, because they indicate various sorts of S's and k's and F's and G's in the different columns, the computations in the following table are easily followed :—

	S°. $k_m = 2.$ $V_p = V_m.$	S°. $k_s = 2{\cdot}07.$ $V_p = V_s.$	$S_{\frac{1}{2}\pi}$. $k_m = 2.$ $V_p = V_m.$	$S_{\frac{1}{2}\pi}$. $k_s = 2{\cdot}07.$ $V_p = V_s.$
$\frac{1}{2}S \cos(\frac{1}{2}V_m - V_p)$	− ·0261	− ·0039	− ·0084	+ ·0140
$\frac{1}{2}S \cos(\frac{1}{2}V_m + V_p)$	+ ·0174	+ ·0622	+ ·0240	+ ·0074
Sum Σ	− ·0087	+ ·0583	− ·0324	+ ·0214
Diff. Δ	− ·0435	− ·0661	+ ·0156	+ ·0066
$k\Sigma$	− ·0174	+ ·1207	− ·0648	+ ·0443
$k\Delta$	− ·0870	− ·1368	+ ·0312	+ ·0137
$\Sigma + k\Delta = F$	− ·0957	− ·0785	− ·0012	+ ·0351
$\Delta + k\Sigma = g$	− ·0609	+ ·0546	− ·0492	+ ·0509
$\frac{1}{2}S \sin(\frac{1}{2}V_m - V_p)$	− ·0059	− ·0030	+ ·0065	+ ·0039
$\frac{1}{2}S \sin(\frac{1}{2}V_m + V_p)$	− ·0915	+ ·0047	− ·0087	+ ·0417
Sum σ	− ·0974	+ ·0017	− ·0022	+ ·0456
Diff. δ	+ ·0856	− ·0077	+ ·0152	− ·0378
$k\sigma$	− ·1948	+ ·0035	− ·0044	+ ·0944
$k\delta$	+ ·1712	− ·0159	+ ·0304	− ·0782
$\sigma + k\delta = f$	+ ·0738	− ·0142	+ ·0282	− ·0326
$-(\delta + k\sigma) = G$	+ ·1092	+ ·0042	− ·0108	− ·0566

Coefficients......	$-A_m$.	$-B_m$	$-A_s$.	$-B_s$.		
$(S^{0}h \cos \frac{1}{2}V_m) - 10 \cdot 50$	$(F_m{}^0)$ $-$ ·0957	$(G_m{}^0)$ $+$ ·1092	$(F_s{}^0)$ $-$ ·0785	$(G_s{}^0)$ $+$ ·0042	=	W.
$(S^{0}h \sin \frac{1}{2}V_m) + 8 \cdot 04$	$(f_m{}^0)$ $+$ ·0738	$(g_m{}^0)$ $-$ ·0609	$(f_s{}^0)$ $-$ ·0142	$(g_s{}^0)$ ·0546	=	X.
$(S\frac{1}{2}\pi h \cos \frac{1}{2}V_m) + 0 \cdot 40$	$(F_m\frac{1}{2}\pi)$ $-$ ·0012	$(G_m\frac{1}{2}\pi)$ $-$ ·0108	$(F_s\frac{1}{2}\pi)$ $+$ ·0351	$(G_s\frac{1}{2}\pi)$ $-$ ·0326	=	Y.
$(S\frac{1}{2}\pi h \sin \frac{1}{2}V_m) + 3 \cdot 74$	$(f_m\frac{1}{2}\pi)$ $+$ ·0282	$(g_m\frac{1}{2}\pi)$ $-$ ·0492	$(f_s\frac{1}{2}\pi)$ $-$ ·0566	$(g_s\frac{1}{2}\pi)$ $+$ ·0509	=	Z.
Multiply by	$-A_m = +30 \cdot 58$,	$-B_m = -38 \cdot 47$,	$-A_s = -21 \cdot 08$,	$-B_s = -3 \cdot 53$.		
$-10 \cdot 50$	$-2 \cdot 91$	$-4 \cdot 23$	$+1 \cdot 66$	$-0 \cdot 01$	=	W.
$+ 8 \cdot 04$	$+2 \cdot 24$	$+2 \cdot 36$	$+0 \cdot 30$	$-0 \cdot 19$	=	X.
$+ 0 \cdot 40$	$-0 \cdot 04$	$+0 \cdot 42$	$-0 \cdot 74$	$+0 \cdot 12$	=	Y.
$+ 3 \cdot 74$	$+0 \cdot 86$	$+1 \cdot 91$	$+1 \cdot 20$	$-0 \cdot 18$	=	Z.

W.		X.		Y.		Z.	
+	−	+	−	+	−	+	−
	10·50	8·04		0·40		3·74	
	2·91	2·24			0·04	0·86	
	4 23	2·36		0·42		1·91	
1·66		0·30			0·74	1·20	
	0·01		0·19	0·12			0·18
1·66	17·65	12·94		0·94	0·78	7·71	
	1·66	0·19		0·78		0·18	
	−15·99	+12·75		+0·16		+7·53	

$$W = -15{\cdot}99 \qquad X = +12{\cdot}75$$
$$Z = +\ 7{\cdot}53 \qquad Y = +\ 0{\cdot}16$$
$$W+Z = -\ 8{\cdot}46 \qquad X+Y = +12{\cdot}91$$
$$W-Z = -23{\cdot}52 \qquad X-Y = +12{\cdot}59$$
$$\tfrac{1}{2}(W+Z) = -\ 4{\cdot}23 \qquad \tfrac{1}{2}(X+Y) = +\ 6{\cdot}46$$
$$\tfrac{1}{2}(W-Z) = -11{\cdot}76 \qquad \tfrac{1}{2}(X-Y) = +\ 6{\cdot}30$$

(q.) *Computation of Astronomical and other Constants.*

Find s_o, the moon's mean longitude (see 'Nautical Almanac,' Moon's Libration), and h_o the sun's mean longitude (sidereal time reduced to angle) from the 'Nautical Almanac,' and p_o the longitude of moon's perigee, from Baird's Manual,* Appendix Table XII (there called ϖ), at the epoch 0^h, January 1, 1887, Bombay mean time, in E. Longitude $4^h{\cdot}855$.

From Baird, Tables XIV, XV, XVIII, find N the longitude of Moon's node, and I, ν, ξ at mid-period, February 14, 1887.†

With the value of I find f_m from XIX (1) for the tides M_2, N, L; from XIX (3) find f_o for the tide O; from XIX (8) find f' for the tide K_1; from XIX (9) find f'' for the tide K_2; from XX find ν' for the tide K_1; and from XXI find $2\nu''$ for the tide K_2.

The results are

$$s_o = 359^\circ{\cdot}43, \qquad h_o = 280^\circ{\cdot}63, \qquad p_o = 165^\circ{\cdot}36.$$
$$\nu = -9^\circ{\cdot}60, \qquad \xi = -9^\circ{\cdot}00.$$
$$1/f_m = 0{\cdot}9709, \qquad 1/f_o = 1{\cdot}161, \qquad f' = 0{\cdot}915, \qquad f'' = 0{\cdot}802.$$
$$\nu' = -6^\circ{\cdot}30, \qquad 2\nu'' = -11^\circ{\cdot}75.$$

Then compute initial equilibrium arguments, in the symbol for which the subscript letters indicate the tides referred to,—

$$u_m = 2(h_o-\nu)-2(s_o-\xi), \qquad u_o = (h_o-\nu)-2(s_o-\xi)+\tfrac{1}{2}\pi, \qquad u_s = 0^\circ,$$
$$= 203^\circ{\cdot}60, \qquad\qquad = 3^\circ{\cdot}37,$$
$$\text{for } K_1,\ u' = h_o-\nu'-\tfrac{1}{2}\pi, \qquad \text{for } K_2,\ u'' = 2h_o-2\nu'',$$
$$= 196^\circ{\cdot}93, \qquad\qquad = 213^\circ{\cdot}01,$$
$$u_n = u_m-(s_o-p_o), \qquad u_l = u_m+(s_o-p_o)+\pi,$$
$$= 9^\circ{\cdot}53, \qquad\qquad = 217^\circ{\cdot}67,$$
$$u_p = -h_o+\tfrac{1}{2}\pi = 169^\circ{\cdot}37.$$

* 'Manual for Tidal Observations,' by Major Baird. Taylor and Francis, Fleet Street, 1886.

† In making these reductions I have really used the value of N for July 1, 1887, because I am operating on tidal *predictions* made for the whole year 1887, which were doubtless made with mean N for that year. The difference is almost insensible.

We have already shown in B the way of computing Π, and $\Pi = 1\cdot034$.*

In C and D we have shown how to compute j, i, l, and $j = +6^\circ\cdot46$, $i = +6^\circ\cdot52$, $l = -0^\circ\cdot97$.

By the formula in B, with $\alpha_n = 86^\circ\cdot97$ for 6 semi-lunations,

$$\omega = 2h_o - 2\nu'' + \alpha_n = u'' + \alpha_n$$
$$= 299^\circ\cdot97 = -60^\circ\cdot03.$$

By the formula in D, with $\beta_n = 87^\circ\cdot52$ for XIII quarter-lunar periods.

$$\theta = 2h_o - \nu' + l + \beta_n,$$
$$= 294^\circ\cdot11 = -65^\circ\cdot89.$$

By the formula in B, viz. :—

$$U \cos\phi = \Pi + \lambda_n \mathrm{f}'' \cos\omega,$$
$$U \sin\phi = \qquad \lambda_n \mathrm{f}'' \sin\omega.$$

With $\log\lambda_n = 9\cdot2517$ for 6 semi-lunations, and with the above values of Π, f″, ω :—

$$\phi = -6^\circ\cdot40, \qquad (+)\log U = 0\cdot0464.$$

By the formula in D, viz :—

$$\mathrm{T}\cos\psi = \mathrm{f}' - \rho_n \cos\theta,$$
$$\mathrm{T}\sin\psi = \qquad \rho_n \sin\theta,$$

with $\log\rho_n = 9\cdot4618$ for XIII quarter-lunar periods, and with the above values of f′ and θ:—

$$\psi = -18^\circ\cdot35, \qquad (+)\log \mathrm{T} = 9\cdot9241.$$

(r.) *Final Evaluation of* M_2.

From (j) $\mathrm{B}_m = +38\cdot47, \quad \mathrm{A}_m = -30\cdot58, \quad \tan\zeta_m = \dfrac{\mathrm{B}_m}{\mathrm{A}_m}$;

B_m is + and A_m is −, so that ζ_m lies in second quadrant; whence

$$\zeta_m = \pi - 51^\circ\cdot51 = 128^\circ\cdot49.$$

Then
$$\mathrm{H}_m = \frac{1}{\mathrm{f}_m}\cdot \mathrm{B}_m \operatorname{cosec}\zeta_m;$$

* As the Indian tide predicting instrument takes no account of solar parallax, I should in reality have done better to take Π as unity. But of course this consideration does not apply to real observations.

whence, on reducing from inches to feet,

$$H_m = 3{\cdot}98 \text{ ft.}$$

Also $$\kappa_m = \zeta_m + u_m = 128^\circ{\cdot}49 + 203^\circ{\cdot}60 = 332^\circ{\cdot}09,$$

where the value of u_m is taken from (q).

(s.) *Final Evaluation of* N *and* L.

Taking the values of P, Q, R, S from (i),

$$f_m H_n \sin(\zeta_n - j) = -P = -10{\cdot}72, \quad f_m H_l \sin(\zeta_l + j) = +Q = +0{\cdot}06,$$
$$f_m H_n \cos(\zeta_n - j) = -S = +\ 7{\cdot}16, \quad f_m H_l \cos(\zeta_l + j) = -R = +1{\cdot}22.$$

$\zeta_n - j$ lies in 4th quad., $\zeta_l + j$ lies in 1st quad.;

whence $$\zeta_n - j = -56^\circ{\cdot}27.$$

Then $$H_n = \frac{1}{f_m}\operatorname{cosec}(\zeta_n - j) \times (-P);$$

whence, on reducing from inches to feet,

$$H_n = 1{\cdot}04 \text{ ft.}$$

Again, since from (q) $j = +6^\circ{\cdot}54$, we have $\zeta_n = -49^\circ{\cdot}73 = 310^\circ{\cdot}27$, and $\kappa_n = \zeta_n + u_n = 310^\circ{\cdot}27 + 9^\circ{\cdot}53 = 319^\circ{\cdot}80$, where the value of u_n is taken from (q).

Turning to the second pair of equations,

$$\zeta_l + j = 28^\circ{\cdot}4.$$

Then $$H_l = \frac{1}{f_m}\sec(\zeta_l + j) \times (-R);$$

whence, on reducing from inches to feet,

$$H_l = 0{\cdot}11 \text{ ft.}$$

Again, since $j = +6^\circ{\cdot}5$, we have $\zeta_l = 21^\circ{\cdot}9$, and $\kappa_l = \zeta_l + u_l = 21^\circ{\cdot}9 + 217^\circ{\cdot}7 = 239^\circ{\cdot}6$, where the value of u_l is taken from (q).

(t.) *Final Evaluation of* S_2 and K_2

From (k) $B_s = +3{\cdot}62$, $A_s = +21{\cdot}08$; $\tan\zeta_s = \dfrac{B_s}{A_s}$;

B_s and A_s are +, so that ζ_s lies in 1st quadrant;

whence $$\zeta_s = 9^\circ{\cdot}71.$$

Then $$H_s = \frac{A_s \sec \zeta_s}{U},$$

whence, with log U already found in (q) as 0·0464, and, reducing inches to feet,

$$H_s = 1·60 \text{ ft.}$$

Again $$\kappa_s = \zeta_s + \phi = 9°·71 - 6°·40 = 3°·31,$$

where the value of ϕ is taken from (q).

Lastly,

$$H'' = 0·272 H_s = 0·44 \text{ ft.}, \qquad \text{and } \kappa'' = \kappa_s = 3°.$$

The factor 0·272 is an absolute constant.

(u.) *Final Evaluation of* K_1, O, P.

Taking the values of $\frac{1}{2}(W-Z)$, $\frac{1}{2}(X+Y)$ from (p),

$$TH' \sin(\zeta' + i - \psi) = \tfrac{1}{2}(W-Z) = -11·76,$$

$$TH' \cos(\zeta' + i - \psi) = -\tfrac{1}{2}(X+Y) = -6·47;$$

$\zeta' + i - \psi$ lies in third quadrant, and

$$\zeta' + i - \psi = \pi + 61°·2 = 241°·2.$$

Then since, from (q), $\psi = -18°·35$, we have $\zeta' + i = 222°·8$; and since from (q) $i = 6°·52$, therefore $\zeta' = 216°·3$; whence

$$\kappa' = \zeta' + u' = 216°·3 + 196°·9 = 53°·2,$$

where the value of u' is taken from (q).

Then $$H' = \frac{\frac{1}{2}(W-Z)}{T} \operatorname{cosec}(\zeta' + i - \psi),$$

whence, with log T already found in (q) as 9·9241, and reducing from inches to feet,

$$H' = 1·33 \text{ ft.}$$

Also $$H_p = 0·331 H' = 0·44, \qquad \text{and } \kappa_p = \kappa' = 53°.$$

The factor 0·331 is an absolute constant.

We now have to compute

$$L = \tfrac{1}{2}(X+Y) \tan \tfrac{1}{2}\epsilon + f'H' \cos(\zeta' + i) \tan \tfrac{1}{4}\epsilon,$$

$$M = \tfrac{1}{2}(W-Z) \tan \tfrac{1}{2}\epsilon - f'H' \sin(\zeta' + i) \tan \tfrac{1}{4}\epsilon,$$

where $\log \tan \frac{1}{4}\epsilon = 9{\cdot}0677$, an absolute constant for all times and places. With the values of f' and $\frac{1}{2}(X+Y)$ and $\frac{1}{2}(W-Z)$ given above in (q) and (p), and with the values of H' and $\zeta'+i$ just found, there results—

$$L = -0{\cdot}495, \qquad M = -0{\cdot}214.$$

Now
$$f_o H_o \sin(\zeta_o - i) = -\tfrac{1}{2}(W+Z) + L,$$
$$f_o H_o \cos(\zeta_o - i) = \tfrac{1}{2}(X-Y) + M.$$

We have found in (p)

$$\tfrac{1}{2}(W+Z) = -4{\cdot}23, \qquad \tfrac{1}{2}(X-Y) = +6{\cdot}30,$$

so that

$$f_o H_o \sin(\zeta_o - i) = +3{\cdot}73, \qquad f_o H_o \cos(\zeta_o - i) = +6{\cdot}09.$$

Whence $\zeta_o - i$ lies in the first quadrant, and

$$\zeta_o - i = 31^\circ{\cdot}50.$$

Then
$$H_o = \frac{1}{f_o}\left[\tfrac{1}{2}(X-Y) + M\right] \sec(\zeta_o - i),$$

whence, reducing from inches to feet,

$$H_o = 0{\cdot}69 \text{ ft.}$$

Again,

$$\kappa_o = \zeta_o + u_o = (\zeta_o - i) + i + u_o = 31^\circ{\cdot}50 + 6^\circ{\cdot}52 + 3^\circ{\cdot}37 = 41^\circ{\cdot}39,$$

where the value of u_o is taken from (q).

(v.) *Final Reduction of Mean Water Mark.*

We subtracted 99 inches from all the heights before using them, and the mean of the heights was then + 3·51 inches. Hence mean water is 102·51 inches, or 8·54 feet above the datum of the original tidal observations.

(w.) *Results of Reduction.*

		Mean of 9 yrs. obs.	Error of present calc. in inches and minutes.
Mean water, 8·54 ft.		8·223	4 in.
M_2	$H = 3·98$ ft.	4·043	$\frac{3}{4}$ in. too small.
	$\kappa = 332°$	330°	4^m too fast.
S_2	$H = 1·60$ ft.	1·625	$\frac{1}{4}$ in. too small.
	$\kappa = 3°$	3°	Nil.
K_2	$H = 0·44$ ft.	0·405	$\frac{1}{2}$ in. too large.
	$\kappa = 3°$	352°	22^m too fast.
N	$H = 1·04$ ft.	0·997	$\frac{1}{2}$ in. too large.
	$\kappa = 320°$	313°	11^m too fast.
L	$H = 0·11$ ft.	0·088	$\frac{1}{4}$ in. too large.
	$\kappa = 240°$	308°	$2^h\ 18^m$ too slow.
K_1	$H = 1·33$ ft.	1·396	$\frac{3}{4}$ in. too small.
	$\kappa = 53°$	45°	32^m too fast.
O	$H = 0·69$ ft.	0·658	$\frac{1}{3}$ in. too large.
	$\kappa = 41°$	48°	29^m too slow.
P	$H = 0·44$ ft.	0·404	$\frac{1}{2}$ in. too large.
	$\kappa = 53°$	43°	40^m too fast.

The second column is given because, if the calculation had been conducted by rigorous methods instead of approximately, my results should have agreed very nearly* with these. The causes of several of the discrepancies are explicable. The error of mean water mark is due to the necessity for neglecting the annual and semi-annual tides in a short series of observations. The error in the height of S_2 is partly due to my taking $\Pi = 1·034$ instead of putting it equal to 1, as is virtually done in the Indian tidal instrument. If I had taken $\Pi = 1$, I should have had $H_s = 1·65$ nearly. The error in phase in K_2 is a necessary incident of the shortness of the series of observations. The error of N must be due to the shortness of the series, which has not permitted an adequate elimination of the evectional and variational tides. The tide L is only about an inch in height, and accuracy of result could not be expected.

The magnitude of the error in time in the diurnal tides is disappointing, but it is clear that the length of observation has not been sufficient to disentangle the O tide from the K_1 tide. It may be remarked also that an error of 1° in phase makes twice as much differ-

* I do not know the exact values of the constants used in the Bombay Tide Table, which has been used as representing observation.

ence in time with the diurnal tides as with the semi-diurnal. The errors of P fall under the same category as those of K_2.

Lastly it is probable that all these errors would have been sensibly diminished if I had subtracted 103 inches from the heights all through instead of 99, and I know that this is to some extent the case.

(x.) *Verification.*

In a calculation of this kind some gross error of principle may have been committed, such, for example, as imputing to some of the κ's a wrong sign; and this is the kind of mistake which is easily overlooked in a mere verification of arithmetical processes. It is well, therefore, to test whether the tide heights and times are actually given by the computed constants. This is conveniently done by selecting some three or four tides from amongst those from which the reductions have been made, and it makes the calculation much shorter if we pick out cases in which it is H. or L.W. within a few minutes of noon.

For example, in the present case it was L.W. on February 16 (day 46) at $0^h\ 7^m$ P.M., and the height was 4 ft. 0 in.; again, it was H.W. on March 25 (day 83) at $0^h\ 3^m$ P.M., and the height was 13 ft. 3 in.

Now, if U denotes the value of any equilibrium argument whose value at the epoch, 0^h, January 1, was denoted in (q) by u, and if A_0 denotes the height of mean sea-level above datum, the expression for the height of water is:—

$$
\begin{aligned}
h = A_0 &+ f_m H_m \cos(U_m - \kappa_m) + H_s \cos(U_s - \kappa_s) + f'' H'' \cos(U'' - \kappa'') \\
&+ f_m H_n \cos(U_n - \kappa_n) + f_m H_l \cos(U_l - \kappa_l) + f' H' \cos(U' - \kappa') \\
&+ f_o H_o \cos(U_o - \kappa_o) + H_p \cos(U_p - \kappa_p).
\end{aligned}
$$

The time of H.W. depends on a formula involving the sines of the same angles in place of cosines.

Since we have chosen cases where it is H. or L.W. at noon, the U's exceed the u's by an exact number of days' motion.

The evaluation of the separate terms may be conveniently made by means of an ordinary nautical traverse table, where (neglecting the decimal point) fH is represented by the "Distance," and $fH\cos(U-\kappa)$ is given by "Latitude," and $fH\sin(U-\kappa)$ by "Departure."

If we know the time of H. or L.W. within 20^m or so, the following calculation will give the true time and height. The computation applies to the first of the two cases where we know that there should be a L.W. at about 0^h of day 46. The increments of argument are computed from the Table G, and the κ's are subtracted either by actual subtraction or by addition of $2\pi - \kappa$.

	M_2.	S_2.	K_2.	N.	L.	K_1.	O.	P.
Increment in 40^d	$464^\circ \cdot 7$		$78^\circ \cdot 9$	$-57^\circ \cdot 9$	$-452 \cdot 7$	$39 \cdot 4$	$425 \cdot 3$	
Ditto 6^d	$-146 \cdot 3$		$11 \cdot 8$	$-224 \cdot 7$	$-67 \cdot 9$	$5 \cdot 9$	$-152 \cdot 2$	(see K_1)
Ditto 46^d	$318 \cdot 4$		$90 \cdot 7$	$-282 \cdot 6$	$-520 \cdot 6$	$45 \cdot 3$	$273 \cdot 1$	$-45 \cdot 3$
$u =$	$203 \cdot 6$		$213 \cdot 0$	$9 \cdot 5$	$217 \cdot 7$	$196 \cdot 9$	$3 \cdot 4$	$169 \cdot 4$
$U =$	$522 \cdot 0$		$303 \cdot 7$	$-273 \cdot 1$	$-302 \cdot 9$	$242 \cdot 2$	$276 \cdot 5$	$124 \cdot 1$
$-\kappa =$	$-332 \cdot 1$	$-3 \cdot 3$	$-3 \cdot 3$	$+40 \cdot 2$	$+120 \cdot 4$	$-53 \cdot 1$	$-41 \cdot 4$	$-53 \cdot 1$
$U-\kappa =$	$189 \cdot 9$	$-3 \cdot 3$	$300 \cdot 4$	$-232 \cdot 9$	$-182 \cdot 5$	$189 \cdot 1$	$235 \cdot 1$	$71 \cdot 0$
$U-\kappa =$	$\pi+10$	-3	-60	$\pi-53$	$\pi-3$	$\pi+9$	$\pi+55$	71

		f.	H.	fH.	$U-\kappa$.	fH cos $(U-\kappa)$. +	fH cos $(U-\kappa)$. −	fH sin $(U-\kappa)$. +	fH sin $(U-\kappa)$. −
Semidiurnals.	M_2	1·03	3·98	4·10	$\pi+10^\circ$		4·04		0·71
	S_2	1·0	1·60	1·60	-3	1·60			0·08
	K_2	0·80	0·44	0·35	-60	0·18			0·30
	N	1·03	1·04	1·07	$\pi-53$		0·64	0·86	
	L	1·03	0·11	0·11	$\pi-3$		0·11	0·01	
						+1·78	−4·79	+0·87	−1·09
							+1·78		+0·87
						$A_2 =$	−3·01	$B_2 =$	−0·22
Diurnals.	K_1	0·915	1·33	1·22	$\pi-9$		1·21		0·19
	O	0·86	0·69	0·59	$\pi-55$		0·34		0·48
	P	1·00	0·44	0·44	71	0·14		0·42	
						+0·14	−1·55	+0·42	−0·67
							+0·14		+0·42
						$A_1 =$	−1·41	$B_1 =$	−0·25
						$\frac{1}{4}A_1 =$	−0·35	$\frac{1}{2}B_1 =$	−0·13
						$A_2+\frac{1}{4}A_1 =$	−3·36	$B_2+\frac{1}{2}B_1 =$	−0·35

$$\text{Time} = -120^m \left(\frac{B_2+\frac{1}{2}B_1}{A_2+\frac{1}{4}A_1}\right) = -120^m \times \frac{35}{336} = -12^m = 11^h\ 48^m \text{ A.M.}$$

Tabular time $= 0^h\ 7^m$ P.M.
Error $= -19^m$.

$A_2 + A_1 =$	−4·42
Mean water	+8·54
Height =	4·12

Height L.W.........	4 ft. 1 in.
Tabular height......	4 0
Error...............	+1

In the second case referred to above, the calculated height is 13 ft. 6 in., and the tabulated height 13 ft. 3 in., and the error in time is -15^m.

These results are as good as might be expected, and, considered as a prediction, would be amply sufficient for navigational purposes.

XII. "An Experimental Investigation of the Central Motor Innervation of the Larynx. Part I. Excitation Experiments." By FELIX SEMON, M.D., F.R.C.P., and VICTOR HORSLEY, B.S., F.R.S. (From the Laboratory of the Brown Institution.) Received June 17, 1890.

(Abstract.)

In this paper the authors communicate the first part of a series of researches in which they have been engaged from time to time since 1886, on the nature of the representation of the intrinsic laryngeal movements in the central nervous system.

Briefly stated, the *anatomical* arrangement of the laryngeal nerve centres they believe to be as follows:—

a. Cortical areas of representation.

b. Connecting fibres in the corona radiata and the internal capsule.

c. Bulbar areas of representation.

So also the *physiological* differentiation existing in the functional activity of these centres and fibres they regard as to be viewed from two standpoints—

1. The *phonatory* laryngeal movements.
2. The *respiratory* laryngeal movements.

Of these the former are shown to be especially represented in the cortex, and the latter more particularly in the bulb.

After a complete historical *résumé* of the experimental, and also clinical, work already done on the subject, the authors describe and discuss the experimental procedure adopted by them, with especial reference to the complications introduced in the employment of varying intensity of stimulation, depth of anæsthesia, species, individual peculiarities, and age of the animal.

The results are then arranged in order, according to the part stimulated, beginning with the cortex cerebri and ending with the bulb.

It is shown that in the cortex cerebri there is represented the *phonatory* movement of adduction,* and that this is more completely developed, the higher the animal is in the scale of evolution.

Further, that in the neighbouring regions of the cortex the respiratory movements of the larynx, acceleration, intensification, and slowing, are also represented. Only in one kind of animal, viz., the cat, were a focus and area of pure cortical representation of abduction

* Discovered by Ferrier and accurately localised in the dog by Krause.

observed. Notice is taken of the meaning of the acceleration or polypnœa thus evoked, and of its relation to the thermotaxic function suggested.

Excitation of the corona radiata and the internal capsule resulted in the mapping out of the fibres conducting the above motor effects downwards, and it is suggested that the localisation by previous observers of basal centres for the functions above mentioned is possibly to be accounted for differently in the light of these observations.

Finally, the results of exploring the floor of the fourth ventricle by excitation are described, so far as intrinsic effects were produced in the larynx. The representation of adduction and abduction movements respectively was thus localised.

In conclusion, the relations of the various parts of this central mechanism to one another are shortly discussed.

XIII. "Contributions to the Molecular Theory of Induced Magnetism." By J. A. Ewing, F.R.S., Professor of Engineering in University College, Dundee. Received June 18, 1890.

As the facts of induced magnetism become better known, increasing interest attaches to molecular theories and increasing difficulty attends the theories that are current. Weber's fundamental conception that the molecules of iron or nickel or cobalt are always magnets, and that the process of magnetising consists in turning them from many directions towards one direction, has been strongly confirmed by the now well established fact that there is a true saturation value, a finite limit to the intensity of magnetism, which may be reached or very closely approached by using a strong magnetic force.* Without going further back, to enquire (with Ampère) how the molecules come to be magnets, we may take this conception as the natural starting point of a theory. But when we go on to examine the conditions of constraint on the part of the rotatable molecules which have been suggested to make the theory square with what is known about permeability, about residual magnetism and other effects of magnetic hysteresis, about the effects of stress, of temperature, of mechanical vibration, and so forth, we find a mass of arbitrary assumptions which still leave the subject bristling with difficulties. Many of the phenomena suggest, for instance, the idea that there is a quasi-

* Ewing and Low, 'Phil. Trans.,' 1889, A, p. 221. See also H. E. J. G. du Bois, 'Phil. Mag.,' April, 1890.

frictional resistance which opposes the turning of the molecular magnets; this notion lends itself well to account for the most obvious effects of magnetic hysteresis and the reduction of hysteresis by vibration. On the other hand, it conflicts with the fact that even the feeblest magnetic force induces some magnetism. My object in this paper is to refer to another (and not at all arbitrary) condition of constraint which not only suffices to explain all the phenomena of hysteresis without any notion of friction, but seems to have in it abundant capability to account for every complexity of magnetic quality.

In describing Weber's theory, Maxwell points out that, if each molecular magnet were perfectly free to turn, the slightest magnetic force would suffice to bring the molecules into complete parallelism, and thus to produce magnetic saturation. He continues, "This, however, is not the case. The molecules do not turn with their axes parallel to the force, and this is either because each molecule is acted on by a force tending to preserve it in its original direction, or because an equivalent effect is produced by the mutual action of the entire system of molecules. Weber adopts the former of these suppositions as the simplest."*

Weber supposes a directing force to act in the original direction of the molecule's axis which continues to act as a restoring force in that direction after the molecule is disturbed. This assumed constraint is quite arbitrary; moreover, if it were the only constraint, there would be no residual magnetism when the deflecting force was withdrawn. Accordingly, Maxwell modifies Weber's theory by introducing the further assumption that when the angle of deflection exceeds a certain limit the molecule begins to take permanent set. The development of this, however, does not agree well with the facts.

The alternative which is offered in the sentence I have quoted from Maxwell was not followed up by him, and seems to have been very generally overlooked, notwithstanding its obvious freedom from arbitrary assumption. Several writers, notably Wiedemann† and Hughes,‡ have recognised the inter-molecular magnetic forces by suggesting that the molecules, when unacted on by any magnetising force from outside, may form closed rings, or chains, "so as to satisfy their natural attraction by the shortest path."§ But Wiedemann expressly postulates a frictional resistance to rotation, which will prevent this arrangement from being more than approximately attained, and which may be more or less overcome by vibration.‖

* Maxwell, 'Electricity and Magnetism,' vol. 2, § 443.

Wiedemann, 'Galvanismus,' 2nd ed., vol. 2 (1), p. 373.

‡ Hughes, 'Roy. Soc. Proc.,' May 10, 1883.

§ Hughes (*loc. cit.*).

‖ Wiedemann, 'Phil. Mag.,' July, 1886, p. 52; 'Elektricität,' vol. 3. §§ 784—785.

I lately commented on the fact that soft iron and other magnetic metals (notably nickel under particular conditions of strain)* show a remarkably close approach to instability at certain stages in the magnetising or demagnetising process.† When the magnetic force reaches a particular value, the rate of change of magnetism with respect to change of force may become enormous. Referring to this in a paper which has just been published,‡ Mr. A. E. Kennelly has reverted to the idea of chains of magnetic molecules held together by the inter-molecular magnetic forces, and contends that when such a chain is ruptured by applying a sufficiently strong external magnetic force it will fall to pieces throughout, and the molecular magnets which compose the chain will take their alignment suddenly. He accordingly sketches what he calls a "chain-theory" of magnetisation and an adaptation of the theory of Hughes, in which, however, he postulates an elastic resistance to the rotation of the molecules in addition to the constraint afforded by their mutual magnetic forces. Mr. Kennelly's remarks are highly interesting and suggestive; but I do not think (for reasons which will appear immediately) that the notion of closed magnetic chains can be maintained as a general account of the molecular structure of unmagnetised iron.

I have experimented on the subject by making a model molecular structure consisting of a large number of short steel bar magnets, strongly magnetised, each pivoted like a compass needle upon a sharp vertical centre and balanced to swing horizontally. We cannot readily imitate in a model the two degrees of rotational freedom possessed by actual molecular magnets, but a group of magnets swinging in one plane gives a sufficiently good general idea of the nature of the equilibrium which is brought about by intermolecular forces, and the manner in which that equilibrium is disturbed when an external force is applied. The model is very easily made. Each magnet is a piece of steel wire about one-tenth of an inch in diameter and two inches long (fig. 1), bent in the middle to bring the centre of gravity below the point of support. The hole or rather recess for the pivot is made by a centre punch: the pivot itself is a sewing needle fixed upright in a small base plate which is punched out of a sheet of lead. The bars swing with but little friction, and their pole strength is sufficient to make the mutual forces quite mask the earth's directive force when they are set moderately near one another. The group is arranged on a board on which lines are drawn to facilitate regularity in grouping when that

* See a paper by H. Nagaoka, 'Journal of the Science College of the University of Tokio,' vol. 2, 1888, p. 304.

† 'Journal of the Institution of Electrical Engineers,' No. 84, 1890, pp. 38—40.

‡ 'The Electrician,' June 7th and 13th, 1890.

Fig. 1.

is wanted, and the board slips into a large frame or open sided flat box wound round the top, bottom, and two sides with a coil through which an adjustable current may be passed to expose the group to a nearly homogeneous external magnetic force. The coil is wound in a single very open layer, through which a sufficiently good view of the group inside is obtained.* A liquid rheostat with a sliding terminal is used to secure continuity in varying the magnetic force. It is scarcely necessary to add that the magnetic force which is applied to the group is too weak to have any material effect on the magnetism of individual bars. It alters their alignment only, just as a magnetic force alters the alignment of Weber's molecular magnets.

When a number of these magnets are grouped either in a regular pattern or at random, and are left after disturbance to come to rest free from external magnetic force, they of course assume a form which has no resultant magnetic moment, provided the number be sufficiently great—but it is apparent that they do not arrange themselves in closed chains. Any such configuration would in general be unstable. Many stable configurations admit of being formed, and if the magnets are again disturbed and left to settle the chances are much against any one configuration immediately repeating itself. One general characteristic of these configurations is that they contain *lines* consisting of two, three, or more magnets, each member of a line being strongly controlled by its next neighbours in that line, and but little influenced by neighbours which lie off the line on either side. Thus, if there are two magnets simply, they form (as might be anticipated) a highly stable pair, thus:—

$$\rightarrow \ \rightarrow$$

* In showing the experiments, the board with the magnets on it may, of course, be placed in clear view above the coil; the il is then made larger, or a stronger current is used.

With three magnets, two form a line along one side of the triangle joining the fixed centres, and the third lies parallel, or nearly so, facing the opposite way. Four magnets will usually form two lines with directions which lie nearly along two sides of the quadrilateral, but diagonally opposite magnets may pair, leaving the others unattached. Suppose them set at the corners of a rectangle with unequal sides; they may lie in any of these forms—

if the inequality in distance be not too great. All these configurations are stable, and the condition of least energy, while making the first of them the most probable, does not prevent the occasional formation of the others. In a long line, the same condition leads in general to this formation—

→ → → → → → → → →

but it is by no means uncommon to find a line broken into two or more sections, thus—

→ → → → ↗ ↙ ← ← ←

Seven magnets grouped so that the centres of six form a regular hexagon, with one in the middle, have a great variety of possible stable configurations, of which these are examples:—

Fig. 2.

Experimental study of the forms which may be assumed by groups, and of the vibrations which may be transmitted through groups, is interesting, but to pursue it would be beside my present purpose. In all cases, the configuration assumed by a group is such

that there is stability for small displacements, but different positions of the group may be stable in different degrees, and if members of the group be turned through a sufficiently great angle, they become unstable, and fall into a new position of stability, bringing about a partial reconstruction of the lines that characterise the group. Special interest attaches to square patterns, from the fact that iron and nickel (probably cobalt also) crystallise in the cubic system. In a square pattern of many members, we find, in general, lines running parallel with all sides of the square when the group settles without directive force after a disturbance.

Let the group, or collection of groups, be subjected to an external magnetic force, $\mathfrak{H}$, gradually increasing from zero. The first effect is to produce a *stable* deflection of all members except those which lie exactly along or opposite to the direction of $\mathfrak{H}$. This results in giving a small resultant moment to the group (assuming that there was none to begin with), which increases at a uniform or very nearly uniform rate as $\mathfrak{H}$ increases. This corresponds to the first stage in the magnetisation of iron or other magnetic metal (*a*, fig. 3). The initial susceptibility is a small finite quantity, and is sensibly uniform for very small values of $\mathfrak{H}$.

Suppose that, without going beyond this stage, we remove $\mathfrak{H}$; the molecular magnets, not having been deflected beyond the limit of stability, simply return to their initial places, and there is no residual magnetism. This, again, agrees with the fact that no residual magnetism is produced by very feeble magnetising forces. Up to this point, there has been no magnetic hysteresis. But let the value of $\mathfrak{H}$ be increased until any part of the group becomes unstable, and hysteresis immediately comes into play. At the same time, there begins to be a marked augmentation of susceptibility—that is to say, a marked increase in the rate at which aggregate resultant moment is acquired. It is not difficult to arrange groups in which the state of instability occurs with one and the same value of $\mathfrak{H}$ throughout the group. But, in general, we shall have different elementary magnets, or different lines of them, reaching instability with different values of $\mathfrak{H}$. The range of $\mathfrak{H}$, however, which suffices to bring about instability throughout the whole, or nearly the whole, of the members in most groups is not large; we, therefore, find in the action of the model a close analogy to the second stage (*b*, fig. 3) of the process of magnetisation, in which the magnetism rises more or less suddenly, as well as to the first stage (*a*).

During the second stage (*b*), the magnetic elements fall for the most part into lines which agree more or less exactly with the direction of $\mathfrak{H}$. If, at the end of this stage, we remove $\mathfrak{H}$, we find that a very large proportion of the aggregate moment which the group has acquired remains; in other words, there is a great deal of

FIG. 3.

residual magnetism. To take an instance, suppose we have a group lying initially as in fig. 4, and apply a magnetic force, $\mathfrak{H}$, in the direction sketched, the first stage (a) deflects all the molecular

FIG. 4.

magnets slightly, without making any of them become unstable; the second stage (*b*) brings the molecules into the general direction shown in fig. 5 or rather that is the direction they assume when $\mathfrak{H}$

Fig. 5.

is removed, and the residual magnetism contributed by the group is then the sum of their moments resolved along $\mathfrak{H}$. When $\mathfrak{H}$ is acting, the components along $\mathfrak{H}$ are slightly greater, for the molecules are then (stably) deflected through a small angle towards the line of $\mathfrak{H}$.

Let $\mathfrak{H}$ be further increased—we now have the third stage (*c*, fig. 3), which consists in the closer approach to saturation that is caused by the molecules being more nearly pulled into exact line with $\mathfrak{H}$ (fig. 6). Whether there will be instability during the deflection of them from the lines of fig. 5 will depend on the closeness of the poles, and on the inclination of the lines of fig. 5 to the direction of $\mathfrak{H}$ (see below). In some groups, saturation will be complete with a finite value of $\mathfrak{H}$; in others, it will only be closely approximated to. In magnetising any actual specimen of iron, we have, of course, to deal with a multitude of groups the lines to which lie at very various inclinations to $\mathfrak{H}$. If we remove the force $\mathfrak{H}$ at any point in stage (*c*), we find very little, if any, more

Fig. 6.

residual magnetism than was found at the end of stage (*b*). The ratio of residual to induced magnetism is a maximum about the end of stage (*b*), and diminishes as stage (*c*) proceeds. This, again, agrees completely with the observed facts.

There is *some* hysteresis during the removal (whether complete or partial) and re-application of magnetic force, because (provided we have enough groups to deal with) there will be some lines of elements which pass to and fro through a condition of instability during the removal and re-application of the force. For certain inclinations of the line the movements are not reversible.

Suppose, next, that having applied and removed a strong force $\mathfrak{H}$, leaving strong residual polarity, we begin slowly to reverse $\mathfrak{H}$. At first, the effects are slight; presently, however, instability begins, and, as the force is increased within a narrow range, we find the molecules all upset. This is followed by a stage of nearly elastic deflection as saturation is approached. Thus, the well-known general characteristics of cyclic processes are all reproduced in the model (see fig. 8 below).

Again, a small repeated cyclic change of $\mathfrak{H}$ superposed upon a constant value of $\mathfrak{H}$ produces small changes of resultant moment, which are reversible if the change of $\mathfrak{H}$ is very small. This, as Lord

Rayleigh has shown,* is what happens in a magnetic metal, and the susceptibility with respect to small cyclic changes is small in the model, just as it is in the actual solid.

The chief facts of permeability and retentiveness, and hysteresis generally, are therefore at once explicable by supposing that Weber's molecular magnets are constrained by no other forces than those due to their own mutual magnetic attractions and repulsions. No arbitrary constraining forces are required. In the model the centres of rotation are fixed; in regard to the actual solid we may make an equivalent supposition, namely that the distances between the molecular centres do not change (except in so far as they may be changed by strain).

Hysteresis, then, is not the result of any quasi-frictional resistance to molecular rotations; it occurs whenever a molecule turns from one stable position of rest to another through an unstable condition. When it is forced to return it again passes through a condition of instability. This process, considered mechanically, is not reversible; the forces are different for the same displacement going and coming, and there is dissipation of energy. In the model the energy thus expended sets the little bars swinging, and their swings take some time to subside. In the actual solid the energy which the molecular magnet loses as it swings through unstable positions generates eddy currents in surrounding matter. Let the magnets of the model be furnished with air-vanes to damp their swings, and the correspondence is complete.

A regular group of elementary magnets, especially when furnished with air-vanes, gives a good illustration of what has been called magnetic viscosity. When the imposed force $\mathfrak{H}$ reaches a critical value one of the outer members of the group becomes unstable, and swings slowly round; its next neighbours, finding their stability weakened, follow suit, and the disturbance spreads through the group in a way eminently suggestive of those phenomena of time-lag in magnetisation which I have described in a former paper.†

The model shows equally well other magnetic phenomena which presumably depend on the inertia of the molecules, such as the fact that a given force causes more magnetic induction when suddenly applied than when gradually applied, and leaves less residual magnetism when suddenly removed than when gradually removed.

The well known effects of mechanical vibration in augmenting magnetic susceptibility and reducing retentiveness are readily explicable when we consider that vibration will cause periodic changes in the distances between molecular centres. This has not only a direct influence in making the molecular magnets respond more easily to

* 'Phil. Mag.,' March, 1887.

† 'Roy. Soc. Proc.,' June, 1889.

changes of magnetic force by reducing their stability during the intervals when they recede from each other, but tends indirectly towards the same result by setting them swinging.

The effects of temperature which are common to the three magnetic metals may be stated thus :—Let any moderate magnetising force be applied not strong enough to produce anything like an approach to magnetic saturation, and let the temperature be raised. Then the permeability *increases* until the temperature reaches a certain (high) critical value, at which, almost suddenly, there is an almost complete disappearance of magnetic equality. As regards the first effect, it is clear that an increase of permeability is to be expected from the theory; expansion with rise of temperature involves a separation of the molecular centres, and therefore a reduction of stability. As regards the almost sudden loss of susceptibility which occurs at a high temperature, it may do no harm to hazard a rather wild conjecture. We may suppose the molecular magnets to be swinging more or less, the violence of the swings increasing as the temperature rises, until finally it develops into *rotation*. Should this happen, all trace of polarity would of course disappear. The conjecture that the molecular magnets oscillate more and more as the temperature rises, is at least supported by the fact (carefully investigated by Hopkinson* in iron and nickel; data for cobalt also have lately been supplied by du Bois†) that under *strong* magnetic forces rise of temperature reduces magnetism; for with strong forces the molecular magnets are already ranged so that their mean direction is nearly parallel to $\mathfrak{H}$; hence the earlier effect of heat (to diminish stability and facilitate alignment) does not tell, and the increased swinging simply results in reducing the mean value for each molecule of its moment resolved parallel the magnetising force.

Before referring to effects of stress we may consider shortly the stability of a pair or line of magnets, treating each as a pair of poles subject to the law of inverse squares. Take first a single pair of equal magnets with centres at C and C′. The poles P, P′ would lie in the line CC′, but for the imposed force $\mathfrak{H}$, which produces a deflection CC′P′ or C′CP $= \theta$.

Let α be the angle which $\mathfrak{H}$ makes with the line of centres, m the pole strength, and r the half length of the magnetic axis of each magnet. The deflecting moment is

$$2\mathfrak{H}mr \sin(\alpha - \theta),$$

and the restoring moment is

$$\frac{m^2\overline{\mathrm{CN}}}{\overline{\mathrm{PP}'}^2},$$

* 'Phil. Trans.,' 1889, A, p. 443; 'Roy. Soc. Proc.,' June, 1888.

† 'Phil. Mag.,' April, 1890.

FIG. 7.

CN being drawn normal to PP′. The restoring moment at first increases with θ, but passes a maximum at a value of θ which depends on the relation of r to the distance between the centres. The condition of equilibrium is

$$2\mathfrak{H}mr \sin(\alpha-\theta) = \frac{m^2\overline{\mathrm{CN}}}{\overline{\mathrm{PP'}}^3};$$

and as $\mathfrak{H}$ and θ are increased the equilibrium becomes neutral, that is to say, the condition of instability is reached, when

$$\frac{d}{d\theta}\left\{2\mathfrak{H}mr \sin(\alpha-\theta)\right\} = \frac{d}{d\theta}\,\frac{m^2\overline{\mathrm{CN}}}{\overline{\mathrm{PP'}}^3}.$$

These two equations serve to determine the value of $\mathfrak{H}$ and of θ at which instability occurs. If we have to deal with a long line of magnets instead of a single pair, we have to write $2m^2$ instead of m^2 in the restoring moment.

A considerable amount of stable deflection is possible when the distance between the poles is not small compared with r. When the direction of $\mathfrak{H}$ is not much inclined to CC′ (that is, when α has a value approaching 0) there is no instability. In rows having various inclinations to $\mathfrak{H}$, the first to become unstable as $\mathfrak{H}$ is increased will be that for which $\alpha-\theta$ is equal to $\frac{1}{2}\pi$.

If a, the half distance between neighbouring poles in the undeflected position, be small compared with r, there is but little deflection before instability occurs, and in that case, provided α be not small nor nearly equal to π, the occurrence of instability is defined by the condition

$$\frac{d}{d\theta}\frac{\overline{\mathrm{CN}}}{\overline{\mathrm{PP'}}^3} = 0,$$

which is satisfied when $\tan\phi = \frac{1}{\sqrt{2}}$; ϕ being the inclination of PP′ to the line of centres. Hence, with the same proviso (α not nearly equal to 0 or to π, and a small compared with r), the value of $\mathfrak{H}$ which causes instability is

$$\mathfrak{H} = \frac{m}{12\sqrt{3}\,.\,a^2\sin\alpha},$$

for a single pair of magnets, and twice this quantity for the middle members of a long row. This is, of course, least for magnets which lie normal to $\mathfrak{H}$.

In the special case when $\alpha = \pi$ instability occurs when

$$\mathfrak{H} = \frac{m}{8a^2}$$

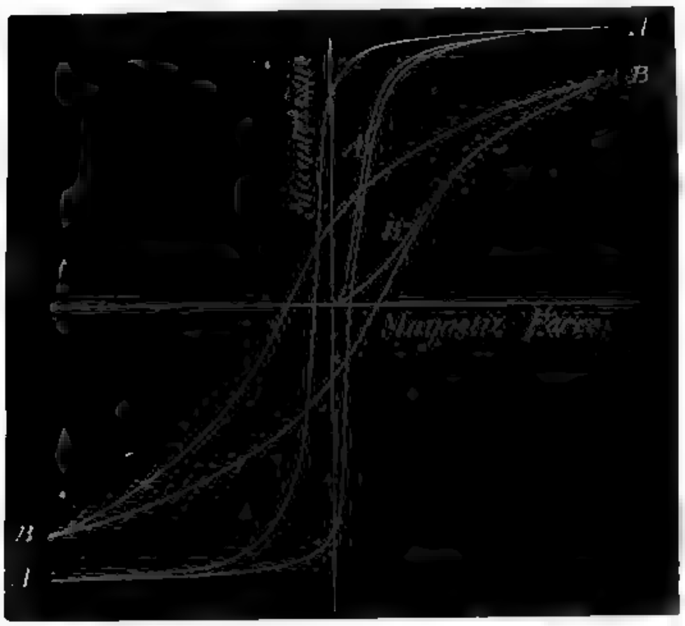
with the single pair, or $m/4a^2$ with the row.

Applied to the case of a group of rows, uniform in distance between the centres, but various as regards their direction with respect to $\mathfrak{H}$, these considerations show that after $\mathfrak{H}$ has reached a value sufficient to make the most susceptible members unstable, no very great increase is required to bring about instability in by far the greater number of the other rows. One general effect of increasing the distance between all the centres is to reduce the range of variation of $\mathfrak{H}$, within which most of the different rows become unstable as the force is progressively increased.

In annealed metal, where we may expect considerable general homogeneity, as regards distance between the centres of the molecular magnets, we find that practically the whole of the abrupt stage in the process of magnetisation is included within narrow limits of magnetising force. We accordingly obtain curves like AA (fig. 8).

When the metal is strained sufficiently to receive permanent set the curves take more rounded outlines (such as BB), showing less susceptibility throughout, less residual magnetism, and more coercive force. The most natural explanation of this, on the basis of the molecular theory, appears to be that mechanical set produces on the whole a shortening of the distances between molecular centres, hence greater stability and more coercive force; but this is associated with hetero-

FIG. 8.



geneity, that is, variety in the distances, hence the rounded outlines of the curves. We know that set tends to develop, or at least to emphasise, heterogeneity; for instance, a bar of iron or steel pulled in the testing machine stretches irregularly after the elastic limit is passed.

The effects of stress and consequent elastic strain on magnetic quality are so complex and so various in iron, nickel, and cobalt that it would be premature to attempt any full discussion of them from the point of view of the theory now sketched. Only a few general features need be referred to at present. Some of these can be traced experimentally in the model by setting the supports of the magnets upon a sheet of thin india-rubber, which may be stretched or distorted to imitate the conditions of longitudinal or torsional strain.

When pulling stress is applied, those rows of molecular magnets which lie more or less along the direction of the stress have their stability reduced by the lengthening of the lines of centres; similarly, rows which lie more or less normal to the stress have their stability increased. The resulting effect on the general susceptibility of the material will depend on which of these conflicting influences preponderates. Let pull be applied before magnetisation begins while the metal is still in a neutral state. The stretching of longitudinal lines

and the contraction of transverse lines will not only alter the stability of those molecules which continue to lie in their original rows, but will tend to make the members of those rows which are much lengthened swing round and form transverse lines in which they will be more stable than before. We may, therefore, reasonably expect that the permeability with regard to strong fields will be reduced by pull, as it actually is both in iron and in nickel, though with regard to weak fields the permeability may be increased, as it is in iron.

Again, the theory explains well why the effects of stress are by no means the same (1) when the stress is applied first and the magnetic force after, and (2) when the magnetic force is applied first and the stress after.

Let a moderate magnetising force be applied, and then begin to apply stress. The first effects are in general large, for the strain precipitates into instability those molecular magnets which were already on the verge of instability. This is beautifully apparent in iron (see 'Phil. Trans.,' 1885, Plates 63 and 64), and the theory shows why the first effects are not reversible, why they do not disappear when the stress is removed, and why it is only in subsequent applications and removals of the stress that the magnetic changes become cyclic.

The same remark evidently applies to the first effects of stress on residual magnetism; also to the first effects of temperature change either on induced or residual magnetism. Again, the theory shows that when a cyclic change of stress is repeated, there will be hysteresis in the corresponding changes of magnetism, whether induced or residual, unless either the cyclic range is very small or the magnetism approaches saturation. During each application of the stress some molecular magnets will swing through unstable positions; during the removal of stress they will swing back, but not at the same values of stress. And it will be characteristic of this hysteresis that the variation in magnetism will begin slowly when the change from applying to removing stress, or from removing to applying stress, begins. All this agrees with the facts.

Moreover, the theory shows that even in the absence of any resultant magnetisation a cycle of stress makes the molecular configuration pass through a series of changes which will at first not be cyclic, but will become cyclic when the stress-cycle is repeated, and in which any intermediate value of the stress will be associated with different configurations during the going and coming parts of the process. In other words, we see that there will be hysteresis in the relation of molecular configuration to stress when a cycle of stress is repeated. Hence certain rather obscure effects which have been observed in magnetic experiments; for instance, where an iron wire is loaded and partially unloaded down to a given load before

being magnetised, its permeability is not the same as when the wire is completely unloaded and reloaded up to the same load. Experimental results of this kind led me in 1884 to write: "If we apply and remove stress in a wire whose magnetic state is entirely neutral, *we cause some kind of molecular displacement in the relation of which to the applied stress there is hysteresis.*"* The theory now offered shows how this happens. Hence also the remarkable hysteresis which the thermoelectric quality of iron exhibits with regard to cyclic changes of stress, discovered by Cohn, and more fully described in 'Phil. Trans.,' 1886, p. 361. The hysteresis of molecular configuration with respect to stress has been proved to be removable or reducible by vibration.

From this theoretical explanation of hysteresis in the effects of stress, it at once follows that a cyclic change of stress (provided it be not very small) involves some dissipation of energy in a magnetic metal, whether the piece be magnetised or not. We may expect this dissipation to be most considerable under conditions which make the magnetic hysteresis large. But it will occur even when there is no external trace of magnetism.

This, of course, implies that in a cyclic process of loading and unloading, work must be spent. There is no perfect elasticity in a magnetic metal, however slowly the process of straining be performed. Under any load there is less strain during application than during removal. This is borne out by experiments on the extension of iron wires ('Brit. Assoc. Rep.,' 1889, p. 502).

The same action occurs to a marked degree in torsional strains. In a twisted specimen there will be a tendency on the part of the molecular magnets to range themselves along lines agreeing more or less with the direction of maximum contraction. Alternate twisting to opposite sides should therefore cause much molecular swinging through unstable positions, with consequent dissipation of energy, even in a piece which is not magnetised.

Without going at present into details, it may be added that the phenomena of molecular "accommodation" studied by Wiedemann and by H. Tomlinson accord with the theory, and that it seems to lend itself well to explain the very remarkable results which have been obtained by Nagaoka† in experiments with nickel wire under twist or under a combination of pull and twist. It also agrees with what little is known as to the influence that previous magnetisation in one direction has upon subsequent magnetisation in another direction.

To sum up, I have endeavoured to show—

* 'Phil. Trans.,' 1885, p. 614.

† 'Journal of the College of Science of the University of Tokio,' vol. 2, 1888.

(1.) That in considering the magnetisation of iron and other magnetic metals to be caused by the turning of permanent molecular magnets, we may look simply to the magnetic forces which the molecular magnets exert on one another as the cause of their directional stability. There is no need to suppose the existence of any quasi-elastic directing force or of any quasi-frictional resistance to rotation.

(2.) That the intermolecular magnetic forces are sufficient to account for all the general characteristics of the process of magnetisation, including the variations of susceptibility which occur as the magnetising force is increased.

(3.) That the intermolecular magnetic forces are equally competent to account for the known facts of retentiveness and coercive force and the characteristics of cyclic magnetic processes.

(4.) That magnetic hysteresis and the dissipation of energy which hysteresis involves are due to molecular instability resulting from intermolecular magnetic actions, and are not due to anything in the nature of frictional resistance to the rotation of the molecular magnets.

(5.) That this theory is wide enough to admit explanation of the differences in magnetic quality which are shown by different substances or by the same substance in different states.

(6.) That it accounts in a general way for the known effects of vibration, of temperature, and of stress upon magnetic quality.

(7.) That in particular it accounts for the known fact that there is hysteresis in the relation of magnetism to stress.

(8.) That it further explains why there is, in magnetic metals, hysteresis in physical quality generally with respect to stress, apart from the existence of magnetisation.

(9.) That, in consequence, any (not very small) cycle of stress occurring in a magnetic metal involves dissipation of energy.

XIV. "On the Relation between the Magnetic Permeability of Rocks and Regional Magnetic Disturbances." By A. W. RÜCKER, M.A., F.R.S. Received May 30, 1890.

[Publication deferred.]

XV. "On the Causes of the Phenomena of Terrestrial Magnetism, and on some Electro-mechanism for exhibiting the Secular Changes in its Horizontal and Vertical Components." By H. WILDE, F.R.S. Received April 22, 1890.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 20th.

Presents, June 19, 1890.

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"On the Germination of the Seed of the Castor-oil Plant (*Ricinus communis*)." By J. R. Green, M.A., B.Sc., F.L.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Communicated by Professor M. Foster, Sec. R.S.

Received January 29,—Read January 30, 1890.

The germination of those seeds in which the non-nitrogenous reserve material is found to be an oil and not a carbohydrate was first studied by Sachs, who, in a series of papers published in 1859,* put forward a hypothesis to explain the manner in which the oil becomes available for the nutrition of the young plant. He records as his starting point the observation that at the onset of germination the oil gradually disappears, just as starch disappears from the reservoirs when germination is started in a seed containing it, and he shows that the development of the young plant proceeds concurrently with this disappearance. From this it follows that the oil, like the starch, is a reserve material to be made use of by the embryo in its early growth. So far as he deals with the changes taking place in the oil, he puts forward the view that starch is directly formed from it, and that this conversion is the first step that may be traced. Subsequently, sugar arises from the starch, and thus, in all seeds alike, the non-nitrogenous reserves travel as sugar from their storehouses to the seat of growth.

Though putting forward the view of the conversion of fat directly into starch, he appears to be scarcely satisfied with it, speaking of it as being very surprising, and, indeed, admitting that in many seeds the greater portion of the disappearing oil gives rise immediately to sugar, and not through the intervention of starch.

Sachs's view that fat or oil is transformed into starch was soon

* 'Bot. Zeitg.,' 1859, col. 178 *et seq.*

after endorsed by Peters,* who published comparative analyses of the oily seed and seedling of the pumpkin. By these tables, Peters shows that during the early period of germination the oil diminished from about 50 per cent. of the weight of the seed till it only amounted to 17 per cent., and that starch appeared in the same time in quantity equal to 4 per cent. of the weight of the seed. During a second period the oil went down from 17 to 11 per cent., while the starch increased from 4 to 7·6 per cent. During a third period, while the oil sank to 4 per cent., the starch also diminished, becoming rather less than 3 per cent. Peters, in considering these analyses, appears to hold the view put forward by Sachs, that the starch is produced by the transformation of the oil, though the quantities given do not at all necessarily bear out the hypothesis.

Fleury,† in 1865, working on the castor-oil, rape, and almond plants, denied the necessity for the occurrence of starch, pointing out that as the oil disappears sugar is to be found. He further noticed that during the germination there was a formation of a non-volatile acid in small quantity.

V. Hellriegel,‡ who investigated rapeseed, agrees with Fleury in denying the occurrence of starch as an intermediate product of the conversion of fat. He points out that germination is attended by processes of oxidation, there being an evolution of CO_2 during the whole period.

In 1871, a new fact was noted by Müntz,§ that during germination a quantity of fatty acid appears in the seed, pointing to a splitting up of the oil into such fatty acid and glycerine. Müntz suggests that the embryo may play a very important part in such a transformation, acting after the manner of a ferment. He failed to find any glycerine in any of his experiments, but says that the fatty acid increases in quantity as the germination proceeds. The fate of the glycerine, if liberated, he did not trace, nor does he suggest what becomes of it.

Schützenberger,‖ in 1876, drew attention to the fact that if oily seeds be steeped in water, an emulsion is obtained in which very soon may be noted the appearance of glycerine and fatty acid, and puts forward the view that during germination an emulsive and saponifying ferment, placed with fat in the presence of water, causes it to undergo true digestion and renders it assimilable. He gives, however, no evidence of the existence of such a ferment.

* Peters, 'Landwirthsch. Versuchsstat.,' vol. 3, 1861.

† 'Annales de Chimie,' ser. 4, vol. 4, p. 38.

‡ Detmer, 'Vergleichende Physiologie des Keimungsprocesses der Samen.' Jena, 1880, p. 334.

§ Müntz, 'Annales de Chimie,' ser. 4, vol. 22, 1871.

‖ 'On Fermentation' (Internat. Scientif. Series, vol. xx).

Detmer,* in 1880, in criticising Müntz's results, quoted above, endeavours to reconcile the decomposition suggested by the latter with the older statements of Sachs as to the origination of starch at the expense of the oil. He suggests that the glycerine may be transformed into certain unknown bodies, and that the fatty acid may be the immediate antecedent of the starch, giving as a possible explanation of the transformation the equation

$$C_{18}H_{34}O_2+27O = 2C_6H_{10}O_5+6CO_2+7H_2O.$$

He had himself, in 1875, published some analyses of hempseed before and after germination, showing that with a disappearance of about 16 parts of oil there was a formation of nearly 9 parts of starch.

In his later work, in 1882, Sachs† somewhat modifies the views originally propounded by him in 1859, though still adhering in the main to his hypothesis that at any rate the greater part of the oil is transformed directly to starch and sugar. He remarks upon the occurrence of oil drops in considerable quantity in the parenchyma of the roots and shoot-axis of the seedlings of *Ricinus* and other plants, and admitting the possibility of the accuracy of Schützenberger's suggestion of ferment action, he thinks that these fat globules may be due to the recombination of the fatty acid and glycerine, which may travel separately from cell to cell, recombining under certain conditions. Such a process presents a certain similarity with the movements of transitory starch.

On the hypothesis of ferment action he suggests that the ferment is formed in the cotyledons and excreted from them into the endosperm.

During all this period, our ideas of the actual transformation of the fatty matter have rested upon hypothesis rather than experiment. Sachs's first work only noted the coincident disappearance of oil and appearance of starch. He does not say that the two processes go on together in the same cells nor even in the same regions of the seed, the starch in *Ricinus* appearing most copiously in the embryo, the oil vanishing from the endosperm. Nor has the ferment, suggested by Schützenberger, been identified, the chief argument for its existence having been derived from Hoppe-Seyler's description of a very similar change in the fats during pancreatic digestion by animals.‡ Nor have we, beyond Detmer's hypothesis already quoted, any attempt to trace the fate of the glycerine and fatty acids formed. The whole question of the mode of the absorption of the fatty reserves of the endosperm by the embryo and their way of passing

* *Op. cit.*

† Sachs, 'Vorlesungen über Pflanzen-Physiologie,' 1882. English Edition. Ward, 1887, p. 347.

‡ Detmer, *op. cit.*, p. 338; Vines, 'Physiology of Plants,' p. 190.

from cell to cell has also remained unexplained, except by Sachs's first hypothesis of their ultimate conversion through the form of starch into sugar, and by a suggestion he makes that some of the oil at least passes as such through the cell walls of the endosperm into the cotyledon.

In taking up the subject where it had thus been left by previous investigators, and endeavouring to throw further light on the several steps that had been established by them, I commenced to work upon the seed of the castor-oil plant (*Ricinus communis*), and I set before me the following problems:—

1st. By what agencies are the reserve materials made available for the nutrition of the embryo?

2nd. In what condition and by what process do they undergo absorption?

3rd. What parts are played in the process of germination by the endosperm and by the embryo respectively?

I selected *Ricinus* as the material to work upon, because the contents of the seed have been accurately ascertained, and found to contain a large quantity of oil; and because the process of germination is not prolonged. A few of the points that arose during the work I also investigated on the seed of the cocoanut palm (*Cocos nucifera*).

The seeds of the castor-oil plant consist of a central embryo, embedded in a mass of endosperm, the whole incased by a hard testa. The micropyle is protected by a well-developed caruncle. A section of the endosperm shows the cells to be nearly filled with oil, which exudes on pressing the bruised seed. This oil can be removed by solvents, when the cells are found to contain also stores of proteid in the form of aleurone grains embedded in protoplasm, the grains containing each a crystalloid of proteid and a small aggregation of mineral matter. Starch is not present, nor is more than a trace of sugar to be found, and this small amount is not constant. There is no great amount of cellulose, the cell walls being very thin and the cells of fair size. No glucoside can be detected in the cells.

The amount of oil to be found is stated differently by different observers. According to Harz,* the quantity varies from 40 to 68 per cent., the highest figure being quoted by Cloez. Blumenbach puts it at 50 per cent. I found a fair sample of the seeds to yield 58 per cent. of their dry weight. The oil is somewhat complex, yielding, on saponification, according to Lecanu and Buffy,† three fatty acids, ricinstearic, ricinic, and ricinoleic, of which the latter is the most abundant. It has the composition represented by the formula $C_{18}H_{34}O_3$.

* 'Landwirthsch. Samenkunde,' 1885, p. 836.

† Harz, 'Landwirthsch. Samenkunde,' 1885, p. 836.

The proteids were investigated in detail by Vines,* who speaks of them as consisting of members of the peptone and globulin classes, the former being soluble in water, the latter in solutions of NaCl.

I. *Agencies by which these reserve materials are made available for the use of the Embryo.*

The first two problems divided themselves at once into two sections, the first of which involved the changes in the oil, the second those in the proteids. They were investigated separately.

(i.) *The Oil.*

Both Müntz and Schützenberger had suggested the probability of the changes in the oil being due to the action of a ferment. My own work on the proteolytic changes in the germination of the lupin† proved a similar cause in connexion with the proteids of that plant. In endorsing Schützenberger's suggestion, Sachs, in 1882,‡ put it forward as probable, from his investigations on the seed of the date (*Phœnix dactylifera*), that the embryo of the young plant, on the onset of germination, formed and excreted ferments to change the reserve materials into a form suitable for absorption.

Some seeds of *Ricinus* were germinated for five days, until the young plants had developed a good root system, and about half the endosperm had disappeared. The seeds were then taken up, the endosperms separated from the cotyledons, and the cotyledons from the hypocotyledonary portion of the embryo. Extracts were made of the endosperms and of the cotyledons, the fluids used being 5 per cent. salt solution and glycerine, in both of which ferments are known to be soluble. Putrefaction was obviated when the salt solution was used by the addition of 0·2 per cent. of potassic cyanide, which was found efficacious in preventing the appearance of bacteria. In the course of the investigation several such extracts were made, the salt solution ones being, on the whole, the most satisfactory.

Experiments were then made to ascertain whether, in the extracts, anything was present which was capable of starting chemical changes in the oil, and, if so, whether that body was of the nature of a ferment. The change first to be expected was the splitting up of the oil into fatty acid or acids and glycerine. 5 c.c. of the extract of the endosperms was mixed with 10 c.c. of an emulsion of castor oil, and set aside in a test-tube in a bath or incubator, at a temperature of 37° C. A control was kept by preparing a similar tube after boiling

* 'Journal of Physiology,' vol. 3, pp. 93—114.

† 'Phil. Trans.,' B, 1887, pp. 39–59.

‡ *Op. cit.*

the 5 c.c. extract. No change was apparent for some hours, but gradually the unboiled tube became acid, while the control remained unchanged. Different extracts varied somewhat in the amount of acidity thus caused, but the difference between the boiled and unboiled tubes was soon evident. A fairly typical experiment is subjoined:—

Tube F was prepared by mixing the extract and the emulsion of castor oil in the proportions given above, and was put into an incubator at 12.30 o'clock on August 22, 1888. A boiled control was put with it, labelled F_1. Both were carefully made neutral. At 4 P.M. 10 drops litmus solution were added to each. F was acid, F_1 neutral. The degree of acidity was in this short time only slight, and addition of 0·1 c.c. of Na_2CO_3 solution of 0·3 per cent. strength again neutralised it. Replacing the tubes in the incubator and leaving them till next morning, F had again become acid. A further addition of the same alkali was made to the contents of this tube, and neutrality was obtained when 1·75 c.c. had been added. The action had been, during the longer exposure, more than proportionately vigorous. No change took place in the control tube.

A similar set of experiments with the extract of the cotyledons failed to produce any evidence of ferment action, the reaction of the liquids always remaining unchanged.

So far as the oil is concerned, therefore, the experiments confirm the hypothesis of Schützenberger, that *there is a ferment in the seeds which can develop fatty acid from the oil.*

Another experiment was then carried out, extending over a longer time, dialysing tubes being used instead of glass vessels. The extracts were prepared by salt solution (5 per cent. NaCl + 0·2 per cent. KCN), and were dialysed before use till nearly free from NaCl, this being found to hinder the action somewhat. The proportion of KCN was kept constant during the experiment.

One fluid ounce of castor oil was made into a thick emulsion with gum and 10 c.c. of the dialysed extract stirred into it. This was put into a dialysing tube and suspended in 200 c.c. of a solution containing 0·6 per cent. NaCl and 0·2 per cent. KCN (No. 1). A control was prepared in the same proportion, boiling the 10 c.c. extract (No. 2). The two were kept in the incubator for a week. During this time the emulsion in No. 1 became gradually purple, and then reddish; that in No. 2 remained blue. The reactions of the dialysates did not change.

On concentrating the two dialysates at the end of the experiment, glycerine was detected in that of No. 1, while No. 2 contained none.

The ferment in the extract had liberated fatty acid and glycerine, and both had become traceable. It was noteworthy, too, that the body causing the acidity had not passed through the dialyser.

The ferment found, therefore, like the ferment in the pancreatic juice of animals, is capable of decomposing fats into fatty acids and glycerine.

The activity of this ferment, like that of so many others, was found to be largely influenced by the reaction of the medium in which it was caused to work. A set of experiments on this point is subjoined:—

Five tubes, A—E, were taken, and in each 5 c.c. of a ferment extract, carefully neutralised, were set to work on 5 c.c. emulsion of castor oil. They were made of different reactions, as under:—

A contained	0·066	per cent.	HCl.
B "	0·133	"	"
C "	0·066	"	Na_2CO_3.
D "	0·66	"	"

E was left neutral, as much water being added to it as was added to each of the others with their respective acid or alkali. A_1, B_1, C_1, D_1, E_1 were controls prepared like the others, but with boiled extract.

After 3½ hours they were examined by the addition to each of 10 drops solution of litmus, when A and E became pinker than their controls, B about equal to B_1. C was less alkaline than C_1, but the D set were relatively unchanged. Titration of the tubes with weak alkali as before showed that *the ferment works best in neutral solution, is hindered by as little as* 0·066 *per cent. of HCl, and stopped by* 0·133 *per cent. of HCl. It is hindered by alkalis also, but works quite well in them if the solution is weak,* 0·066 *per cent. of Na_2CO_3 only retarding the action slightly;* 0·66 *per cent., however, stopped it entirely.*

Neither the 0·133 per cent. HCl nor the 0·66 per cent. Na_2CO_3 destroyed the ferment, for, on neutralising the four tubes, B, B_1, D, D_1, and allowing them to stand in the incubator for several hours, action began again in both the unboiled ones. The energy of the two was, however, very different, for the acid liberated by the ferment in D was five times the amount set free in B in the same time. The activities of B, D, and E, tested during eighteen hours, are represented by the following figures:—

B.	D.	E.
Exposed to 0·133 per cent. HCl for 3½ hours, and then neutralised.	Exposed to 0·66 per cent. Na_2CO_3 for 3½ hours, and then neutralised.	Kept neutral all the time.
10.	50.	85.

The ferment was by similar experiments found to be absent from the resting seeds. Analogy with other ferments of both animal and vegetable origin pointed to its probable existence here in the form of a zymogen. To test this point, several experiments were made. Some resting seeds were ground and treated with ether to extract

the oil. Half the powder was then extracted by the usual solution of NaCl and KCN, while the rest was extracted in the incubator by weak acetic acid, only so strong as to be just perceptible by the tongue. These two extracts were filtered, and labelled respectively A and B, B being then carefully neutralised. Part of A was then acidified with weak acetic acid, and warmed to 35° C. for three hours, and then neutralised. This was labelled C.

On testing them all with castor oil emulsion in the usual way, side by side with boiled controls, A developed no acid, B and C both showed its liberation, giving evidence of the development of ferment by the weak acetic acid. This behaviour is precisely similar to the influence of the same reagent on extracts of the resting pancreas. On allowing extract A to stand in the laboratory for nine days and then again testing it, there was evidence of the presence of ferment, liberated by keeping it. It was not, however, so active as extract C, where the transformation had been brought about by the acetic acid.

(ii.) *The Proteids.*

The oil is only a part of the reserve material present in the seeds, and as so much proteid matter also is present in their cells, some experiments were made to see if a further ferment is present having a proteolytic function.

The proteid in the seed had been stated by Vines* to be a mixture of peptone and globulin, the crystalloid consisting of the latter.

Some ground resting seeds were extracted with water and subsequently with salt solution. Both extracts were found to give a coagulum on boiling, which was most copious in the second case, and separated out completely if a little HNO_3 was added. This was Vines's globulin. After filtering off this precipitate while hot, another proteid was found to be left in solution, which was precipitated as the liquid cooled, and redissolved when it was warmed again. There was much less of this than of the first one. No dialysable proteid was present. The first proteid found by Vines was therefore not a peptone, as he supposed, but an albumose.†

The method adopted in searching for a proteolytic ferment in the germinating seeds was exactly the same as the one I had used in the case of the lupin,‡ and the results showed that again here, as there, a tryptic ferment is developed during germination, which can split up fibrin with formation of peptone and crystalline bodies, including tyrosin.

* *Loc. cit.*

† *Cf.* Martin "On the Proteids in Papau-juice." 'Journal of Physiology,' vol. 6, p. 354.

‡ 'Phil. Trans.,' B, 1887, pp. 39—59.

Thus *the germinating seeds are seen to contain at least two ferments, both working towards the utilisation of the reserve materials in the nutrition of the embryo.*

The glyceride ferment is capable only of producing fatty acid and glycerine from the oil. The changes cannot, however, end with this decomposition. Fleury had noticed in 1865* that there was free acid formed during germination. Müntz had pointed out that the reaction of the embryo was strongly acid, and that at no stage in the germination could he detect glycerine in any of the tissues.† Again, sugar is present in the germinating endosperms in abundance.

Before making experiments on the possible decompositions of the bodies resulting from the action of the ferments, it seemed advisable to examine more carefully the contents of the endosperm. Some seeds were germinated till the endosperms were on the point of separating from the cotyledons; they were then removed, and the endosperms ground up in a mortar with distilled water. This solution was strongly acid. It filtered nearly clear, but was a little opalescent, which appearance was found under the microscope to be due to very minute drops of oil or fatty acid. The solution was then dialysed with distilled water for two days, when the dialysate was found to be acid in reaction. The power of dialysis of the free fatty acids was presumably small, they being extremely greasy liquids, very much like the oil itself. Careful experiments showed that they had no power of dialysis, or at most an extremely feeble one, whether tested as they were or made up into an emulsion. After two days' exposure in a dialyser, the outside liquid had the merest trace of acidity. The experiment noted above,‡ when the digestion was carried out in a dialyser, also negatives the theory of their being able to pass a membrane, for, though glycerine passed out during the experiment, the action of the dialysate remained neutral. As, then, ricinoleic acid will not dialyse, there was evidently another acid in the germinating endosperm. The dialysate of its extract was next evaporated to dryness on a water-bath, and the residue extracted with ether. A certain amount of insoluble matter was left, and the extract therefore filtered. On slowly evaporating the ether, it deposited a crystalline residue, which was freely soluble in water with a resulting acid reaction.

An extract of the cotyledons gave exactly similar results. This new acid was really the cause of the acid reaction of the whole of the tissues of the young plant, already shown to exist by several of the writers already quoted.

The extracts themselves, of both endosperms and cotyledons,

* *Op. cit.*

† *Op. cit.*

‡ P. 375.

deposited on evaporation a residue which gave up to ether a little oily matter, as well as this crystallisable acid, differing thus from the dialysates. In both extracts evidently there existed a mixture of the crystallisable acid with oil and fatty acid.

The fatty acids are also present in the cells. A further quantity of the germinating endosperms was extracted by ether for two days, and the liquid decanted and filtered. A greasy residue was left, the greater part of which was insoluble in water. Unlike the oil, it was largely soluble in 0·2 per cent. NaHO. A solution in this reagent, when neutralised by HCl, gave a white curdy precipitate. This was soluble again in more alkali, and was again thrown down on acidifying. This behaviour was exactly reproduced by some pure ricinoleic acid supplied by Schuchardt.

The extract of the cotyledons contained a little, but not much, of this fatty acid. It seemed likely that a good deal of the contents of the disintegrating endosperm cells might be adhering to the cotyledons, and another experiment was therefore made, using cotyledons that had been carefully washed in 0·2 per cent. NaHO to remove any such *débris*. This weak soda solution, after the washing, gave an opalescence with HCl, and the extract made of the washed cotyledons was found to contain a very faint trace of the fatty acid.

The germinating endosperms, tested with iodine under the microscope, were found to contain no starch. There was starch only in certain regions of the young embryo. On testing a piece of the germinating endosperm with Fehling's fluid a copious reduction was observed. The extracts made and used as described above also gave evidence of the presence of sugar. In the extract of the ungerminated seeds very much less was present. 22 grams of these were found on careful titration to contain 0·025 gram, or 0·11 per cent. of sugar, calculated as dextrose. The quantitative examination of the germinating seeds showed a great increase, which appeared to proceed side by side with the disappearance of the oil. 18 grams of dry weight of endosperms were taken and thoroughly extracted by ether. The seeds from which the endosperms were picked had a root now about 3 inches long, with secondary roots attached. The loss of weight after the ether extraction was 4·7 grams, or about 26 per cent. The ether extracted not only the oil that remained unchanged, but certain of the products of its decomposition, viz., the fatty acid and the crystallisable acid then present in the endosperm. The resting seeds contained 58 per cent. of oil, so that 32 per cent., calculated on the weight of the endosperm, or 55 per cent. of the total oil, had disappeared, while some of that which was still traceable had been at least partially decomposed. The endosperm was next thoroughly exhausted with boiling absolute alcohol, which extracted all the sugar present. This was evaporated to a syrup, dissolved in water,

and titrated, when it was found to contain 0·174 gram of sugar, reckoned as dextrose, or 0·97 per cent. of the original dry weight. Without taking into account the amount that had been absorbed, which must have been very considerable, as the young plant had attained a great degree of development, there was still present almost ten times as much as in the resting seed.

Examination of the proteid constituents in the germinating endosperm, showed that in addition to the undecomposed proteids found in the resting seeds peptone was present. The quantity obtainable from different extracts was not uniform, some being very rich and others containing but little. The cotyledons were removed from some of the advanced seeds and extracted with water in dialysers. After two days the dialysates were concentrated to small bulk, and acetate of zinc added. This caused a precipitate, which was filtered off, washed, and suspended in water. The zinc was removed by H_2S, and the watery solution concentrated again, when it deposited crystals of asparagin.

The endosperm, examined in the same way, was found to contain no perceptible amount of this substance.

On examining some of the endosperms after a prolonged period of germination, carried indeed so far that there was only a thin, almost slimy casing over the cotyledons, the cells were found to be empty of solid contents, except a thin layer of protoplasm, and the cell walls were disintegrating and disappearing. No substances could be extracted now, except a small amount of sugar and some of the crystallisable acid described above. Absorption was still proceeding, though the young plant had attained a considerable development.

The germinating endosperm was thus found to contain oil, free fatty acid, a crystallisable acid, a greatly increased quantity of sugar, peptone, and unaltered proteids. No glycerine was present. The embryo contained also a certain amount of asparagin. These bodies are not all due to the action of the ferments. The latter cannot decompose the fatty acid, nor produce the dialysable one. Nor can the sugar be traced to its activity. The first problem then is partially solved by the identification of the ferments, but the work of the glyceride one, at least, needs supplementing by further activity connected with vital processes taking place in the endosperm cells under conditions to be discussed later,* and under the influence of the protoplasm of these cells.

II. *Mode of Absorption of the Reserve Materials.*

The form and manner in which these different reserve materials are absorbed has, as before mentioned, been the subject of hypothesis.

* *Cf.* p. 384 *et seq.*

Sachs* held that, as the cells of the cotyledons of *Ricinus* contain oil after germination has commenced, this oil must have the power of passing through cell walls, and even through the epidermis of the cotyledons.

Detmer,† on the other hand, suggests that, like starch, it becomes a material capable of dialysis, and travels by such means, the oil which appears in the cotyledons and other parts being due to a re-formation at the spot at which it is found, as is the case with transitory starch.

The structure of the cotyledon shows that the actual passage of oil into it would be a matter of very great difficulty. Its outer epidermis is separated from the cells of the endosperm by a very thick layer of cell walls, the remains of cells whose contents have been absorbed; and microscopic examination of this layer while absorption is proceeding fails to detect any fat in its thickness. All analogy points too to dialysis as the mode of absorption, and the forms in which the reserve materials are to be found in the endosperm during germination indicate such a process as the probable one.

Of the various bodies found, sugar and the crystallisable acid are easily capable of dialysis, and are obtainable from the cotyledons. Peptone also can pass through a membrane. Asparagin can be detected on the cotyledonary side, but not in the endosperm. Peptone, however, is not to be detected in the cotyledons.

Besides these, there are to be found in the young plant a certain amount of oil, some considerable quantity of starch, and a trace of fatty acid.

The form of absorption of the nitrogenous matter seems to be that of asparagin, for peptone, though formed, does not seem to leave the endosperm. This is in accordance with the condition in the lupin,‡ where the nitrogenous matter travels in the same way, peptone being only a stage in its formation. The fact that asparagin is not traceable in the endosperm is not a valid objection to this view, for it is quite possible that it is absorbed as fast as formed, or that so little is left behind that it escapes observation.

Detmer's hypothesis seems to account satisfactorily for the appearance of the oil in the young plant, and, if valid, it explains also the trace of fatty acid there. The latter could only be explained otherwise by its having, either in the free state, or in the form of one of its salts, the power of dialysis. Experiments already recorded negative the idea of its dialysing in the free state. Some careful investigations were made as to the behaviour of its alkaline salts. Some ricinoleic acid was made into soaps with different strengths of

* *Op. cit.*, p. 347.

† *Op. cit.*, p. 370.

‡ Green, *op. cit.*

soda solution, and these were dialysed for two days in freshly tested dialysers. I expected to find that, instead of dialysing intact, there would be a decomposition, and that the fatty acid would be left behind, while the alkali escaped. Contrary to this expectation, the dialysates in all cases gave a marked opalescence, or a curdy precipitate, with HCl, the ricinoleic acid being liberated thereby from soap which had passed the dialyser. Subsequent careful examination of the dialysing tube proved it to be intact. The experiment, though not without interest, does not throw any light on the mode of absorption of the acid, for, on neutralising or making slightly acid, the soap is decomposed, and the fatty acid liberated. As the reaction of both endosperm and cotyledon is acid, it is clear that the fatty acid does not pass from the one to the other in the form of a soap.

The occurrence of the starch must be similarly explained. It results, as in other plants, from the transformation of the sugar which has been absorbed. In sections of the hypocotyledonary portion of the axis I found some roundish bodies, which when heated with iodine stained brown. With a $\frac{1}{10}$ objective these brownish bodies were seen to contain small crescent shaped bodies which were dark blue. Some contained two, others three or four, of these. There is little doubt that these were amyloplasts, containing starch grains of very small size, but in course of formation. I did not succeed in identifying these in the cotyledons, though starch appeared there in small grains. Gris* has figured bodies exactly corresponding to these, and he says he finds them in the cells of the epidermis of the cotyledon.

An examination of the relative quantities of these different conditions of the fatty reserve materials present at different stages of the germination confirmed the view given above. The disappearance of oil and coincident increase of sugar have already been commented on. The relations between the oil and the fatty and crystallisable acids were separately determined. Some seeds were germinated in an incubator, and samples were examined at intervals of twenty-four hours from their being sown. Care was taken to have all the seeds of about the same size, and as much alike as possible. Two sets of experiments were made, in one the quantities of the different constituents of the whole seed and resulting plant being examined—in the other those in the endosperms alone.

Each sample was crushed and extracted by ether, the extract being evaporated to dryness, and the amount of residue roughly estimated. This residue was then stirred well with water, which was syphoned off, and added for twenty-four hours to the remains of the crushed endosperms now freed from the ether. The extract so obtained was examined for crystallisable acid and for sugar. The

* 'Ann. des Sci. Nat.,' Ser. 5, *Bot.*, vol. 2, 1864.

residue left by the ethor, after being washed with water as described to remove any soluble acid, was treated with weak alkali (0·2 per cent. NaHO) to remove fatty acid, and this alkali afterwards acidified by HCl to set free the fatty acid from the soap formed. Coincidently with each experiment, endosperms of the same age were examined for glycerine.

The following tables give the results of the experiments:—

Table I.—Whole Plant examined.

Time germinating.	Degree of development of embryo.	Residue deposited by the ether.	Oil found in same.
hours 24	Seeds just cracking testa	Copious, greasy......	Bulk of residue.
48	Primary root 1 cm. long	"	75—80 per cent.
72	Lateral roots emerging	Slightly less in bulk, greasy	50 per cent., about.
96	Root system spreading	Still less. Water mixed with the fat	20—30 per cent.
120	Large root system...	Less still. Getting watery	About half last quantity.
168	Endosperm nearly all absorbed	All soluble in water	None.

Time germinating.	Fatty acid found in same.	Acid soluble in H_2O found in embryo.	Sugar.
hours 24	Traces only	25 c.c. extract neutralised by ·9 c.c. of ·2 per cent. NaHO	Trace.
48	20—25 per cent.	25 c.c. required 1·65 c.c. acid	More.
72	50 per cent., about.	25 c.c. required 2·3 c.c. acid	Fair reaction with Fehling's fluid.
96	70—80 per cent.	25 c.c. required 2·6 c.c. acid	Good reaction.
120	Half residue......	Very acid reaction, did not test quantitatively	About as last.
168	None	Strongly acid	About as last.

Two or three points of interest appear in this table. The gradual disappearance of the oil is accompanied by a rise in the quantity of fatty acid up to the fourth day. This then diminishes in turn, and at the end of the period, while still some endosperm is left, both have

disappeared. The amount of material extracted by the ether is about the same for the first three days, when it gradually and regularly diminishes, and at the same time gradually changes its character, becoming much more acid, and containing increasing quantities of water. The endosperm altogether is much more watery at that stage, a certain amount coming away with the ether, and having to be separated by decantation before evaporating the latter. At the conclusion of the period, a considerable amount of acid is left in the endosperm, which is soluble in water and in ether. The quantities of this acid and of the sugar increase up to the fourth day, and then remain fairly constant, a slight further increase only being noticed in the acid. This is to be accounted for by the rapid development of the plant at about that time, the material leaving the endosperm being that at whose expense this growth takes place.

No glycerine could be detected in the endosperms throughout.

In the second set of experiments, in which the endosperms were separated from the embryos, no quantitative estimation was made of the sugar, as its formation coincidently with the disappearance of the oil had been noted continually.

Taking the endosperms alone as shown in the table (p. 385), they bear out the hypothesis based upon the examination of the whole plant. The acid which was dialysable increased in the endosperms up to the fourth day, and then gradually diminished, pointing to an absorption taking place at a rather faster rate than its formation. Taking the whole plant, the acid increased slightly after this time, showing that it was not used in the growing-points quite so rapidly as it was absorbed, but still was undergoing metamorphosis there.

The fact that at the close of the period during which the endosperm supplies nutriment to the embryo only sugar and dialysable acid are present in its cells, besides a little proteid matter, seems a fair indication that these are the bodies into which the reserve material of the oil is transformed for absorption.

The reserve materials of the resting endosperm are thus found to be all replaced by derivatives which are capable of absorption by dialysis.

There is still left a very important question to discuss. Starting with oil and proteids in the resting seed, we find crystallisable acid, sugar, and asparagin passing into the young embryo, and we note intermediate bodies in the shape of the various fatty acids present in castor oil, and of peptone. We find, further, that glycerine which can be liberated from the oil by the ferment, and by laboratory methods, escapes notice however closely it is looked for. What are the probable decompositions that take place, and how can these explain the various products found?

We have clearly first the splitting of the oil into the fatty acids

Table II.—Endosperms only Examined.

Time of germination.	Water present in the endosperm.	Residue from ether extraction.	Oil found in same.	Fatty acid.	Acid soluble in H_2O. Amounts of 0·2 per cent. NaHO required to neutralise 20 c.c. in each case.
hours 24	None	Copious, greasy	Nearly whole bulk	A drop or two on surface of solution of the soap after adding HCl	1 c.c.
48	None	About same amount and character	A little less	Thin scum, not covering the solution of the soap after adding HCl	1·8 c.c.
72	None	As No. 3 ...	Not much ..	Scum covering the solution treated as before	1·4 c.c.
96	A trace ..	Less in amount and losing greasy character	Few drops..	Thin scum on solution	2·15 c.c.
120	More	Still less watery in character	Trace	Curdy precipitate with HCl, floating to top of solution	1·85 c.c.
168	Good deal	Hardly any oily matter in it. Crystaliisable matter present	Mere trace..	Less than in last	1·7 c.c.
192	As No. 6 .	No oily matter apparent	None	Mere trace	Did not titrate.

and glycerine. That there is a connexion between the former and the acid which passes into the cotyledon seems certain when we compare the formation of the latter as the former disappears, and remember that the fatty acids themselves are not capable of dialysis. The transformation is not brought about by the agency of the ferment, nor is anything excreted by the cotyledon which will bring about the change. It must not be forgotten that the endosperm is the seat of many processes of oxidation, for, as already stated, Detmer has shown the germination is accompanied by a constant evolution of CO_2.

De Saussure has traced out the same process, showing, too, that there is during the germination a constant absorption of oxygen. Even more light is thrown upon the point by the statement of Godlewski,* that this absorption of oxygen is not, as De Saussure believed, greater than the output of CO_2 throughout the whole period. He states that it is not till the radicle protrudes that the oxygen is taken up in greater quantity, and he points out that as the fat disappears from the seed, the inequality between the oxygen absorbed and the CO_2 exhaled gets less and less. It is just at this period that the fatty acid is being replaced by the dialysable one, as shown in the tables given above. That gentle oxidation of the fatty acids is possible is shown by Hazura and Grüssner,† who state that when alkaline solutions of the liquid fatty acids of castor oil are oxidised by permanganate of potash several derivatives are formed, including some of the lower members of the series of fatty acids. Krafft‡ also states that when ricinoleic acid is oxidised with nitric acid, normal heptylic acid is formed, together with azelaic and oxalic acids. Unfortunately, I have not been able to get sufficient quantity of the acid which is formed during germination to enable me to ascertain its identity. The plant itself at that age is very small, and though the reaction of the acid to litmus-paper is very well marked, the quantities occurring in any case are too small for analysis.

The disappearance of the glycerine is, in all probability, to be associated with the appearance of the sugar. The possible sources of the latter are only three, the fatty acids, the proteids, and the glycerine. That sugar results from the former of these is very unlikely. Theoretically there seems to be a possibility of the change, but no laboratory experiments have yet succeeded in bringing it about. Fischer§ has recently shown that certain acids can, by a process of reduction, give rise to sugar, but these are not such acids as those occurring here. The great probability, too, of the fate of the fatty acids being that suggested above weighs heavily against their furnishing the sugar. The proteids, too, can be accounted for in another way, as appears below. There remains, then, only the glycerine.

Fischer has established the fact that the transformation of glycerine into sugar is possible, and the famous experiments of Luchsinger|| upon glycerine as an antecedent of glycogen in the course of the hepatic metabolism bear upon the same point. The ready appearance of sugar is, by this hypothesis, accounted for, and the fact that it appears side by side with the fatty acid in the endosperm

* Pringsheim, 'Jahrb. Botan.,' vol. 13, 1882.

† 'Monatsh. f. Chemie,' vol. 9, pp. 475—484.

‡ 'Deutsch. Chem. Ges. Ber.,' vol. 21, 1888, pp. 2730—2737.

§ 'Deutsch. Chem. Ges. Ber.,' vol. 22, p. 2204.

|| Pflüger's 'Archiv,' vol. 8, 1874, and vol. 18, 1878.

is what would be expected. It is unlikely to be derived from the fatty acid, for the disappearance of a small quantity of this is accompanied by the appearance of a small quantity of the lower acid; it is also unlikely to be derived from the latter, from the fact that both appear synchronously, both increase together, and both are left when the endosperm is finishing its work. Further, the glycerine set free by the decomposition of the oil is sufficient in quantity to account for all the sugar formed.*

There is, of course, another possible alternative. Vines suggests† that the glycerine may undergo oxidation at once with acids, and may be therefore represented by the acids found. This, though possible, seems unlikely, as these last are so much more readily traced to the fatty acid part of the disrupted oil molecule, and especially as we are then reduced to the hypothesis that the sugar comes from the lower acids formed, which has been seen to be improbable.

There is still another theory which needs notice. It is put forward by Vines‡ to explain what Sachs has said, as to starch resulting from the transformation of fat:—"The processes which attend the early stages of the germination of an oily seed may be briefly stated thus: protoplasm undergoes decomposition to form starch, and the continued formation of starch depends upon the reconstruction of protoplasm from the nitrogenous residues of previous decomposition, together with some form of non-nitrogenous organic substance; the non-nitrogenous substance in question is fat." The argument appears unnecessary now that experiments already quoted show that Sachs is in error as to the formation of starch in the endosperm cells during normal germination, but the form of the carbohydrate is not material, and the hypothesis may be advanced to explain the appearance of sugar. It is clear, however, that, if a direct conversion of some antecedent into sugar is possible, it is a much more probable thing than the round-about method suggested. The theory seems called for only to explain an otherwise inexplicable phenomenon, and might as fairly be put forward to explain the appearance of sugar as starch disappears, an appearance which is known to be due to a direct transformation of the latter.

* Since this paper was read, Messrs. Brown and Morris have published in the "Journal of the Chemical Society" (June, 1890) an important research on the germination of some of the Gramineæ. Some of their results have a bearing on the point under notice. They find that excised barley embryoes can be nourished on a solution of glycerine, and under such circumstances a considerable amount of growth takes place in them, which is accompanied by the production of starch in their tissues, just as when they are fed with a solution of sugar. The appearance of carbohydrate at the expense of glycerine is established by their experiments.—J. R. G., July 10, 1890.

† *Op. cit.*, p. 229.

‡ *Op. cit.*, p. 206.

The fate of the resting proteids presents no difficulty. Transformed by the proteolytic ferment into peptone and later into asparagin, we can trace them at once into the cotyledons. There is not a very great store of proteid in the seed; and in the young plant, at any rate before the development of chlorophyll, there is a considerable amount of asparagin. A comparison with the processes in the lupin, where the transformations can be worked out without the complication of the metamorphosis of a large amount of oil, indicates the course of events. In so many other plants, too, the fact of the transport of nitrogenous matter in the form of soluble amides towards the seats of growth has been established, that it seems unnecessary to look for any other form in this case.*

III. *The parts played by the Embryo and the Endosperm respectively in the Process of Germination.*

Allusion has already been made to the statement of Sachs,† that the ferments which cause decompositions in the reserve materials are always formed in the young plant or embryo, and are excreted from the latter into the endosperm. He quotes especially in support of this view the fate of the seeds of *Zea Mais*, and those which, like the date, have their non-nitrogenous reserves laid up in the form of cellulose. Some years before,‡ Gris claimed to have established that the endosperm was, during germination, the seat of an independent life, as much as the embryo was, and was by no means a passive contributor to the latter. Some careful experiments were conducted by Van Tieghem, and published by him in 1877,§ to which no allusion is made by Sachs, but which throw an important light upon this question. Part of his work, like that of Gris, was carried out on *Ricinus*, seeds of which plant were deprived, by careful dissection, of their embryos, and were exposed on damp moss for some weeks to a temperature of 25—30° C. After several days of this exposure he found the isolated endosperms were growing considerably, and at the end of a month they had doubled their dimensions. The change was caused by the enlargement of the constituent cells and the development of air-spaces between them. In the interior of the cells he found the aleurone grains to be gradually dissolving, and the oily matter to be diminishing, though slowly. The dissolution extended throughout the mass of the endosperm, and was not especially prominent on the side that had been nearest to the cotyledons. He

* *Cf.* Sachs, 'Vorlesungen über Pflanzen-Physiologie,' Eng. Trans. by Marshall Ward, p. 346.

† *Op. cit.*, p. 344.

‡ Gris, "Recherches anatomiques et physiologiques sur la Germination." 'Ann. des Sci. Nat.,' Ser. 5, *Bot.*, vol. 2, 1864, p. 100.

§ "Sur la digestion d'Albumen." 'Comptes Rendus,' vol. 84, p. 578.

noted, too, that, though starch did not normally appear in the germinating endosperm, under the condition of non-removal of the products of the decompositions, it did appear in the cells in the form of small grains, though not till after several days. Van Tieghem also observed that the progress of the decompositions could be arrested, and the endosperm made to re-assume a quiescent condition, and that then the aleurone grains again became formed, though in less quantity than before.

To a large extent my observations confirm these of Van Tieghem, though I did not continue the experiment for so long a time. Some endosperms prepared by removal of the embryo by the knife were exposed on damp sand to a temperature of 38° C. in an incubator. Others were placed with them after removal of the plumule and radicle only, leaving the large cotyledons lying on the endosperms undisturbed.

After three days, the former ones contained a little unaltered oil, a good deal of fatty acid, a trace of crystallisable acid, and a little sugar. The latter set were much more swollen than the former, and contained a larger amount of crystallisable acid; the other constituents being much the same.

In a further experiment endosperms, prepared similarly, were attached to the under side of the cork of a small wide-mouthed bottle containing a little water, and were thus cut off from the access of free oxygen. They were then placed in the incubator at 38° C. The change in bulk of these was very slight during the time (fourteen days) during which the experiment lasted; and when examined their contents were found to be much less affected. Part of the oil was transformed, about one-fifth being replaced by fatty acid, while no sugar and no acid soluble in water could be extracted from them.

I cannot confirm Van Tieghem's observation as to the occurrence of starch in the cells under these abnormal conditions; but my experiment, made under the same conditions as his, probably was not conducted for a period long enough to secure its formation. Gris* states that when he found starch grains formed in the endosperm cells it was not until their first contents had been completely absorbed.

Histological investigation of the endosperms, both when germinated normally and when the embryo had been removed, indicates that the changes are not set up by the latter. The decomposition of the oil does not in either case take place regularly on the side nearest to the cotyledons, but cells throughout the whole endosperm are affected simultaneously.

The same conclusion is pointed to by the experiments already quoted as to the antecedent condition of the ferment. The ground

* Gris, *op. cit.*

resting endosperm, when extracted with salt solution or with glycerine, yields up to the solvent something which, though possessing no ferment power, yet is capable of having this developed in it by warming with dilute acid. The cotyledons, on the other hand, are found, even when germination is active, to contain no ferment.

Yet it seems improbable that the function of the young embryo in its relation to the endosperm is one of absorption only. Van Tieghem found that the young endosperm, apart from the embryo, increased in bulk with extreme slowness, not doubling its size till a month had elapsed. In normal germination this result is attained in five or six days. This is not due simply to slowing of the process, owing to elaborated materials not being absorbed. In some of my experiments I laid the flat surfaces of the isolated endosperms upon dialysing paper exposed upon moist sand, so that absorption could take place, but even then the rate of development was scarcely accelerated.

On the other hand, the germination was much more rapid when a small piece of the cotyledon was left adhering to the endosperms, though no removal of products could, under the conditions, take place. The first set of experiments alluded to (p. 375), when seeds with all the embryo taken away were germinated side by side with those that had only lost its axis, shows, too, that the mere presence of the cotyledons, apart from their absorbing power, had a considerable influence on the progress of the germination.

It is difficult to suggest an adequate explanation of this action. As already shown, it is not caused by the formation and subsequent excretion of the glyceride ferment. Nor does it appear that the embryo excretes anything that may bring about the later changes which the ferment does not effect. An embryo extracted from a germinating seed, and washed from adherent matter, was found powerless to effect any change in free ricinoleic acid when the two were placed together in an incubator. Nor would a watery extract of several embryos taken from germinating seeds produce any change in an emulsion of ricinoleic acid in the direction of forming the crystalline dialysable acid found to result from the oxidation of the former. It seems probable that its growth or development acts as a stimulus to the protoplasm of the endosperm cells in which it is embedded, whereby these are caused to undergo their metabolic changes more rapidly than they do in the absence of such stimulus. The processes which are most affected seem to be those dependent on a supply of free oxygen, and not those of the ferment action, a point which supports the idea of the former ones being the expression of the vital activity of the protoplasm of the endosperm cells. Such a stimulus is probably of a physiological character, and is not the mere increase of pressure on the endosperm as the embryo grows.

The investigation of the three points discussed in the foregoing portion of this paper has left unsolved another point of considerable interest, to which my attention was drawn somewhat accidentally. In examining the action of the glyceride ferment upon different emulsions, some experiments were carried out with milk, and there was then found to be present in the germinated seeds not only the two ferments already described, but a third, which acts like rennet. A tube containing 5 c.c. of milk and 2 c.c. of a glyceride extract of germinating endosperms clotted in five minutes when exposed to a temperature of 35° C., a control with the extract boiled remaining unclotted for hours. At the same time a certain amount of acidity was developed in the milk by the action of the glyceride ferment, but the action on the casein was not caused by this. A further experiment was made to establish this conclusion. Two tubes were prepared as before, and their contents coloured with litmus. As soon as clotting had taken place in the one with unboiled extract, dilute HCl was added to the control till it was equally acid with the other, but no clotting or precipitation took place. A further similar set of tubes was prepared, their contents being made slightly alkaline, and on exposure in the incubator the one with unboiled extract clotted, while the control did not.

This ferment, like the other, was by similar experiments shown to be present in an antecedent or zymogen condition in the resting endosperms, and to be capable of conversion in the same manner.

This rennet ferment was found to be most easily extracted by glycerine.

The discussion of the meaning of the rennet ferment here, as in so many plants, must be deferred, pending the completion of further experiments now in progress.

Summary.

The work detailed above leads to the following conclusions :—

1. The reserve materials in the endosperm of *Ricinus communis* consist of oil and proteid matters, the latter being a mixture of globulin and albumose.

2. The changes in germination are partly due to ferment action, there being three ferments present in the germinating seed, one a proteolytic one resembling trypsin; the second a glyceride one, splitting the oil into fatty acid and glycerine; the third a rennet ferment.

3. At least two of these, and therefore, presumably, all of them, are in a zymogen condition in the resting seed, and become active in consequence of the metabolic activity stirred up in the cells by the conditions leading to germination, especially moisture and warmth.

4. The changes caused by the ferment action are followed by others due to the metabolism of the cells, these being processes of weak oxidation.

5. The embryo exercises some influence on the latter, setting up as it develops a stimulus probably of a physiological description.

6. The result of these various processes is to bring about the following decompositions:—

> The proteids are by the ferment converted into peptone, and later into asparagin.
>
> The oil is split by the glyceride ferment into fatty acid and glycerine; the latter gives rise to sugar, and the former to a form of vegetable acid, which is soluble in water and in ether, is crystalline, and has the power of dialysis.

7. The mode of absorption is in all cases by dialysis.

8. The appearance of starch and of oil in the embryo or the young plant is due to a secondary formation, and not to a translocation of either.

"A Note on an Experimental Investigation into the Pathology of Cancer." By CHARLES A. BALLANCE and SAMUEL G. SHATTOCK. Communicated by Sir JAMES PAGET, Bart., F.R.S. Received April 15—Read May 1, 1890. Revised June 10, 1890.

Our first method of conducting the enquiry was by seeing if any special micro-organism could be artificially cultivated from malignant tumours, such as can be done from tubercle, and the pathological formations of certain other infective diseases.

These experiments were made in most instances with carcinomata of the breast, and in a manner fully detailed in the 'Pathol. Soc. Trans.,' vol. 38. We thus experimented with three lipomata, one myxôma, three sarcomata, and about thirty carcinomata.

The results yielded by this particular method, and the particular cultivating media mentioned, may be described in a single word as negative.

We have been able to keep portions of many carcinomata sterile for an indefinite time after various periods of incubation up to thirty-three days; and in one case, in which fluid human serum was employed, the incubation was continued for 134 days.

The pieces transferred to solid media which remain sterile undergo no change perceptible to the naked eye. We have at the present time (February 20th, 1890), amongst others, a piece of a

carcinoma of the mamma 1 inch in length, which was placed on agar immediately after the excision of the tumour on May 12th, 1887. It was incubated at 100° F. for some days and has since been kept in a warm cupboard. It does not even now show any naked-eye change and looks as though it had just been removed.

By the same method we showed the absence of micro-organisms in healthy living tissues.

Notwithstanding such negative results, we do not by any means think that the evidence from analogy that cancer is probably micro-parasitic in origin is hereby overthrown. For of the micro-organisms already known, some are very selective in regard to artificial culture media, others whose existence admits of easy microscopic demonstration have as yet withstood the efforts made to cultivate them without the body. Moreover, it may be allowable with respect to the parasite itself to conjecture that possibly it does not belong to the Protophyta, but to the Protozoa; in which case the difficulty of artificial culture would be easily explained; and the enormous rapidity of cell growth in cancer might be thought of as being induced by a cancerous rejuvenescence setting in in consequence of the conjugation of the "parasite" with the cells of the normal tissues.

And so the culture medium in the case of cancer, it may be, has yet to be found.

Human blood serum apparently offers the most likely chance of success, and with it we have made some experiments. We have obtained human blood serum from fresh placentæ by the method of expression, and have employed it, both liquid and inspissated, at blood heat. After some experience we found it best to have the blood collected in sterile bottles directly from the divided cord whilst the placenta was as yet unexpelled. However, no growth has occurred under sterile conditions either when the serum has been simply inoculated or when a piece of living cancer has been placed in or upon it. The tubes were kept in the incubator for some weeks. The same result has also attended the use as a nutrient soil of fluid or inspissated human hydrocele fluid; *e.g.*, a piece of scirrhus was incubated for eight days on inspissated hydrocele fluid, after which it was kept at the temperature of an ordinary living-room. It has remained sterile and without appreciable change to the present date, twenty months since the experiment was performed.

Abstract of Three of the Experiments with Human Placental Serum.

Experiment 1. *Scirrhus of the Breast.*—Pieces of the tumour were cut with knives which had been wrapped in cotton wool and heated on a previous day in the "iron box" for an hour in the hot-air steriliser at 150° C. The pieces thus cut were put into two tubes of

human placental blood serum discontinuously sterilised and inspissated.

The blood collected, as previously stated, was poured into long sterilised test-tubes, which were allowed to stand in cold water and plugged with sterilised wool. The following day the serum was drawn off with a pipette and transferred to other sterilised tubes plugged with wool. These were then placed on six successive days in a serum steriliser, and some of them afterwards were solidified. The blood furnished from a single placenta was not more than sufficient to charge two or three tubes. Into two tubes, as above stated, of solidified serum thus prepared were placed pieces of the above tumour. One of these showed a white coccus growth on the seventh day; the other remained sterile, and was incubated at blood heat for three months. It was then prepared for the microscope. The microscopic sections show distinct "budding" of both epithelial and connective-tissue cells.

Experiment 2. *Large Recurrent Carcinoma of Breast. Woman, æt.* 30.—Into three tubes of fluid human placental serum were placed pieces of the growth about half an inch in longest measurement. Into three other tubes of solidified human placental serum were placed three other pieces of the growth after the fluid (expressed at the time of solidification) had been poured off. These last three tubes were then partly filled with fluid human placental serum, previously warmed, and prepared as described under Experiment 1. All these tubes were incubated at 100° F. Three weeks later, cover-glass preparations from the tubes showed in all a variable degree of coccus growth. The vitality of the organisms was tested by inoculating tubes of nutrient jelly. In all cases an iridescent growth occurred which when examined proved to be of coccus form. We were surprised at all the tubes showing a growth, and intend to make some further observations on the micro-organism.

Experiment 3. *Small Carcinoma of Mamma.*—Woman advanced in age. Into three tubes one of fluid human placental serum and two of nutrient agar with 6 per cent. glycerine were placed pieces of the growth. Incubation at 100° F. On the eighth day one of the agar tubes showed a "white growth" which was found to be a staphylo-coccus. From each of the other tubes a tube of nutrient jelly was inoculated. A fortnight afterwards neither of these jelly tubes showed any growth. On the eighth day the sterile piece of cancer on agar was transferred to Müller's fluid; on microscopical examination it showed typical capitate processes projecting from the nuclei of many of the epithelial cells, as also free granules in the alveoli; some also of the connective-tissue cells show typical budding. These appearances are not visible in sections of a portion of the same tumour which was hardened in a like manner but not incubated.

The piece of tumour submerged in fluid human placental serum was incubated at 100° F. for 134 days. It is wedge-shaped, about half an inch in longest measurement, and appears now at the end of 134 days quite unchanged; the serum, moreover, is clear and unaltered.

Certain appearances presented on microscopic examination by the portions of the tumours which had remained sterile after various periods of incubation at 100° F. are recorded and figured in the 'Pathol. Soc. Trans.,' vol. 39. These consisted in the extrusion of the chromatin from the nuclei in the form of bud-like processes which ultimately appeared to become free of the cells. Similar appearances were observed in sections of cancerous tumours which had not been incubated, but we failed to observe them in normal tissues which had remained sterile on nutrient media, whether incubated at 100° F. or not.

Transplantation Experiments.—Having carried out a large number of bacteriological experiments from the point of view of the possible infectious nature of cancer, it was necessary to proceed with the inquiry in the direction of inoculation or transplantation experiments on brutes with living cancerous tissues. Wild animals are exempt apparently from the ravages of this disease; but those domesticated not rarely fall victims to it.

Plan of Experiments.—The tumour, immediately after its severance from the body of the patient, was placed in a small incubator at 100° F., and conveyed in a cab to the Brown Institution. There it was allowed to stand in a large incubator, while the animal in whose body it was to be grafted was etherised.

The operation was conducted with strict antiseptic precautions. When the peritoneum had been opened, one of us (S.) took the tumour from the incubator and with a sterilised knife or scissors carefully removed all superfluous tissue (*e.g.*, breast tissue and fat). The whole tumour was then transferred to the abdominal cavity, or several portions were cut from it and pushed in different directions with the finger within the peritoneum. In some instances a piece was fixed by fine catgut in the centre of a muscle (*e.g.*, the biceps), the muscular tissue being brought together with catgut over it, and in a third series of experiments portions of tumour with more or less of the surrounding tissues were placed in the subcutaneous tissue or in the subperitoneal tissue.

We avoided the use of tumours that were ulcerated, for we discovered that, even with very slight surface ulceration, the deeper parts of the tumour (even at 1½ or 2 inches distance) gave an abundant growth in a gelatine tube. As a rule, the operation wounds ran an aseptic course, and the animals were not inconvenienced at all. In a few cases in which the tumours were discovered afterwards to be on the verge of breaking down, septicæmia set in, and the brutes were killed with chloroform.

The tumour tissue was transferred to the body of the animal in from a half to one and a half hour after removal. In the interval it was kept at 100° F., so that its component elements probably maintained their vitality practically unimpaired.

In all cases a small piece of the tumour was placed immediately after the transplantation in Müller's fluid, and subsequently prepared for microscopical examination. In some of the experiments the tumour was large enough to allow also of a small piece or pieces being incubated on blood serum. These were afterwards examined with high powers of the microscope if they remained sterile.

The discovery of the nuclear particles to which we have ventured to give provisionally the name of cancer sperm or carcinozoa, made us anxious to "graft" with pieces of cancer in which this peculiar nuclear state had been induced. This was done without allowing the selected pieces of cancer to cool below blood heat from the time of their removal from the patient to their lodgment in the body of an animal.

So far all the experiments have yielded negative results; but the life history of cancer is so long, that we think, until the animals have survived for at least two years subsequent to the transplantation, it is impossible to know whether they have been infected or not. That this view is tenable and not improbable, is supported by the instances of those diseases, such as actino-mycosis, in which there is no sign of infection at the inoculated spot until very many months have elapsed. In making this statement, we are cognisant of the experiments in which carcinoma is said to have been transferred from one animal to another of the same species; *e.g.*, Hanau, of Zurich,* transferred squamous carcinoma from one rat to another, the inoculated animal dying within three months of the disease.

All our results up to the present time, with human cancer as far as infection is concerned, have been negative. In those animals that have died, the lump of tumour, if small, has been nearly or quite absorbed, or, if large, an ordinary inflammatory capsule has been found surrounding it, and the tumour tissue itself apparently in a state similar to that known as anæmic necrosis.

We preferred the experiments to be of the character of transplantations rather than of inoculations. It appeared that by the method adopted there could be no doubt that living cancerous cells in large numbers were grafted into the animal experimented upon. We have avoided injections into veins, and other doubtful methods, as likely to lead to erroneous conclusions.

* 'Centralblatt für Chirurgie,' No. 42. Dr. Hanau, being in London at the end of March (1890), was kind enough to show us the photographs and microscopical preparations from the rat he had infected, together with sections of the original tumour.

We are much indebted to Mr. Horsley, Professor Superintendent of the Brown Institution, for his kindness in allowing us every facility at the Brown Institution for carrying on our work, and for his ever ready help in many ways and on many occasions.

Short Abstract of the most important of our Transplantation Experiments.

In all the experiments recorded the tumours were removed from the human subject, except in the case of two of the experiments on dogs.

Monkey (1).—Into the abdominal cavity were placed three small pieces of scirrhous carcinoma of breast that had been previously incubated for seven days at 37° C.

Lived 37 days—death from diarrhœa.

After death, the shrunken remains of the grafts were found adherent to the peritoneum.

Monkey (2).—A piece of a scirrhous carcinoma of breast placed in the abdominal cavity; another stitched with catgut into the biceps.

Lived 115 days. Was killed on account of the appearance of a large, lobulated, pinkish swelling at the site of the abdominal wound.

After death, this mass was found by microscopic examination to be a granuloma, in the base of which was a small piece of prolapsed omentum.

No trace of tumour grafts within the abdomen or in the biceps.

Monkey (3).—Transplantation into both biceps muscles and into muscles of outer part of right thigh.

Death from septicæmia on the 6th day.

The tumour was slightly ulcerated. The gelatine tubes showed a rapid and abundant growth.

Monkey (4).—Into the left biceps was stitched a piece of scirrhous carcinoma of the mamma, and another piece into the right thigh. Lived 66 days. Wounds not quite healed. No remains of grafts found. Death from cold.

Monkey (5).—A piece of scirrhous carcinoma stitched into left biceps, and a second placed beneath the skin of the back.

Lived 15 days. Death from cold. Wounds healed.

Grafts encapsuled by connective-tissue, lemon-yellow in colour.

Microscopic sections of the tumour in both situations show it to be the seat of coagulation necrosis. Around the portion in the biceps is a zone of granulation tissue, which is invading the substance of the tumour, the two being intimately mingled at the periphery of the latter. There are no leucocytes or other living cells in the central portion of the graft. Similar appearances obtain in the microscopic sections of the piece of tumour embedded in the subcutaneous tissue of the back.

Monkey (6).—Into the left biceps and beneath the skin of the right side of the chest were placed pieces of a scirrhus of the breast.

Lived 15 days. Death from cold. Superficial suppuration at seat of wounds.

Monkey (7).—Into the abdominal cavity was placed a thick disc of scirrhus about $1\frac{1}{2}$ inch in diameter.

Lived 3 days. Death from intestinal obstruction.

Post Mortem.—No adhesions. Tumour unaltered in appearance.

Monkey (8).—Into the abdominal cavity was placed a whole scirrhus of the breast.

Lived 49 days. Death from diarrhœa. Wound healed. Graft was contained in a circumscribed abscess.

Cat (1).—Into the abdominal cavity was placed an entire scirrhus of the breast, the fat having been clipped off with scissors; the tumour was dipped in carbolic lotion (1 in 100) before insertion. The operation was done within three-quarters of an hour after removal from the patient.

Wound healed by first intention. Animal died 320 days after the experiment. *Post mortem.*—Adherent to the under surface of the liver were the remains of the tumour. This was of a pale yellow colour, and diminished from its original size, very firm in section, fibrous, and presenting islets of softish yellow semi-fluid substance. All the other organs were healthy, except the kidneys, which were granular.

Rabbit (1).—Into the outer muscles of the right thigh was placed a piece of scirrhus of the breast, $\frac{3}{4} \times \frac{1}{4} \times \frac{1}{4}$ inch.

Animal died 541 days later. *Post mortem.*—In the muscle was found a sharply-defined mass, of flattened oval form, 4 mm. in the shorter diameter; this consists of a distinct capsule of fibrous tissue, enclosing particles of hard earthy substance. It would appear, therefore, that the necrosed piece of tumour had undergone calcification. Viscera all healthy; brain not examined.

Rabbit (2).—Into the outer muscles of the right thigh were placed two pieces of a scirrhus of the breast. Lived 6 days. Negative result.

Rabbit (3).—Into the outer side of the right thigh were placed two pieces of scirrhus.

Lived 36 days. *Post mortem.*—The two pieces of scirrhus were lemon-yellow in colour, fairly firm, and in section presented softish yellow areas.

Dog (1).—Into the abdominal cavity was inserted a whole scirrhus of the breast, $1\frac{1}{2} \times \frac{3}{4}$ inch. Superficial part of wound healed by granulation.

Animal quite well 700 days after the experiment. On this day it

was killed. *Post mortem.*—No peritoneal adhesions. No sign of the graft. All the viscera healthy.

Dog (2).—Abdominal section and insertion of a piece of scirrhus $1\frac{1}{4}$ inch in diameter, and $\frac{1}{3}$ inch in thickness. Nine days later the animal was killed with chloroform. *Post mortem.*—Circumscribed abscess around the shreddy remains of the graft adherent to liver and stomach. No peritonitis.

Dog (3).—1*st Experiment.*—Into the abdominal cavity was placed a scirrhus of the breast, $1\frac{3}{4}$ inch in diameter and discoidal.

2*nd Experiment.*—390 days after the 1st experiment, a whole scirrhus of the breast with some of the surrounding fat was placed in the subperitoneal tissue.

3*rd Experiment.*—170 days after the 2nd experiment, into the peritoneal cavity was placed a slice of a subperitoneal round-celled chondrifying sarcoma of humerus, $1 \times 1\frac{1}{2} \times \frac{1}{3}$ inch in volume. This was pushed to the right of the middle line, a similar slice being pushed to the left. The slices were cut in the spray, all surfaces previously exposed being rejected. One of the slices included the growing margin of the growth. Is still alive and well, 650 days after the 1st experiment.*

Dog (4).—1*st Experiment.*—Beneath the skin of the right side of the thorax, by means of a valvular incision, was placed a small scirrhus of the breast, about as large as a horse bean, most of the fat having been previously cut away in the spray. Primary union of wound.

2*nd Experiment.*—240 days after 1st experiment, into the abdominal cavity was placed the greater part of a breast with two scirrhus masses in it. Death six days later from hæmorrhage into the abdominal cavity. *Post mortem.*—No trace of the first graft beneath the skin of the thorax. No tumours in internal organs.

Dog (5).—1*st Experiment.*—Beneath the skin of the back was placed, an hour after its removal from the patient, a square slice ($\frac{1}{2}$ inch along its sides) of a spindle-celled sarcoma taken from amongst the muscles of the thigh.

Wound healed by first intention.

2*nd Experiment.*—Beneath the skin of the back, 400 days after the 1st experiment, was placed a wedge-shaped piece, about three-quarters of an inch in depth, of sarcoma, including the periphery. This was removed from a tumour filling the zygomatic fossa of another dog, which was kept under ether while the graft was taken, the animal being subsequently killed. Warm saline solution was poured over the piece before its insertion beneath the skin. The

* July 8th.—Animal killed 760 days after 1st experiment. *Post mortem.*—No sign of the grafts. Viscera healthy.

post-mortem on the dog from which the graft was taken showed no growths in any of the internal organs.

The wound healed by first intention and 50 days later no trace of the tumour could be felt. The tumour on microscopical examination was seen to be a small round-celled sarcoma containing many giant cells and undergoing calcification in places.*

Dog (6).—*1st Experiment.*—Beneath the skin of the side of the chest was placed a piece of scirrhus of the breast 1 inch in diameter and $\frac{1}{4}$ inch in thickness. The tumour was discharged by suppuration and the wound healed.

2nd Experiment.—112 days after the 1st experiment, an entire scirrhus of the breast was placed in the abdominal cavity, the tumour being sliced into three pieces, which were forced into different situations.

320 days after the 2nd experiment the animal was in good health. On this day it was killed. *Post mortem.*—Omentum adherent to linear cicatrix in abdominal wall. No sign of the graft discernible. No disease of any of the viscera.

Dog (7). Into the abominal cavity was placed a square slice $\frac{1}{2}$ inch along its sides, and $\frac{1}{4}$ inch thick, cut from a tumour which had been a very short while previously excised from the ischiorectal fossa of another dog. A second piece of the same tumour was placed beneath the skin on the right side of the front of the thorax.

Wounds healed by first intention. Animal in good health 270 days afterwards. The tumour on microscopical examination proved to be a squamous-celled carcinoma with cysts.†

Sheep (1).—Beneath the skin of the back under strict antiseptic precautions was placed a square slice from a subperiosteal round-celled sarcoma of the humerus, 2 inches square, and $\frac{1}{3}$ inch in thickness: this was pushed for some inches into the subcutaneous tissue beyond the incision. The piece included at one of its borders the growing margin with the healthy tissues immediately connected with it. Three weeks after the operation there was a fluctuating swelling at the site of the graft, and six weeks after the operation the graft was discharged in the state of necrosis. On the 108th day the animal, apparently in good health, was killed.

Post mortem.—Sheep healthy in every respect.

Sheep (2).—Beneath the skin of the back was placed a slice of scirrhus of the breast with a rim of the surrounding fat, the whole being $1\frac{3}{4}$ inch in diameter and about $\frac{1}{3}$ inch in thickness.

* July 8th.—Animal killed 590 days after the 1st experiment. *Post mortem.*—No trace of the grafts. No sign of cancerous infection.

† July 8th.—Animal killed 400 days after the experiment. *Post mortem.*—No sign of graft. Viscera healthy.

Death occurred about 36 hours after the experiment of ether—collapse of the lungs.

Sheep (3).—Beneath the subcutaneous fat of the back were placed two grafts of a rapidly growing round-celled sarcoma removed from the pectoral region for the third time. The grafts were cut in the spray and then washed in warm sterile salt solution. The larger graft was a thick slice about $1\frac{1}{4}$ inch in diameter, and the smaller was a cubical piece taken from the other half of the tumour. The wound healed by first intention. On the 30th day the animal was quite well, and on palpation no evidence of the grafts could be discovered.*

Besides the above transplantation experiments, we fed two white rats, male and female, with portions of fourteen fresh scirrhus tumours of the breast. In many instances the animals were fed on two successive days with portions of the same scirrhus tumour. These feeding experiments have extended over a period of seven months, and the rats are now alive and well, 250 days from the date of the first experiment. During this time several litters of young rats have been born. These feeding experiments we propose to continue on the same animals. They were undertaken with the view of seeing if infection of the stomach or intestine could be induced in a way similar to that which is set up in some instances by the injection of tuberculous material.

In regard to the literature of the question, Scheuerlen, in the autumn of 1887, read a paper before the Medical Society of Berlin, claiming to have discovered a bacillus in cancer by means of artificial cultures. Francke corroborated this. But subsequent observers, amongst whom is Baumgarten ('Centralblatt f. Bakteriologie u. Parasitenkunde,' vol. 3, No. 13), have shown that these observations were fallacious and due to contamination. Moreover, Rosenbach and Fränkel (at Koch's request) kindly wrote and told us that the swellings induced by Scheuerlen's operations were from their own observations not cancerous.†

Many experimenters have recorded observations with respect to transplantation or grafting of cancer. Here again the results are at

* July 8th.—Animal killed 165 days after the experiment. *Post mortem.*—No sign of local or visceral infection.

In the same way, between three and four months ago, we transplanted living portions of scirrhus tumours into three white rats. At present they show no signs of infection. It may be said, then, that at present there is no evidence that human cancer can be transferred to brutes.—July, 1890.

† Professor Platon I. Kubasoff, of Moscow ('Proceedings of the third General Meeting of Medical Men at St. Petersburg,' 1889, No. 2, p. 41) has asserted that a bacillus can be cultivated on coagulated blood serum which will produce tumours in rabbits and cats after inoculation. It will appear from what is stated in the text that this requires confirmation.

variance. Senger* and Senn† have never observed tumour formation to ensue after grafting. This accords with the results of earlier observers in this field, Sir William Savory and Sir John Simon. In none of these experiments were the animals suffered to live any considerable time, and it is in this that they are not satisfactory. Of carcinoma it is especially true that it is a disease of advancing years.

In experiments made from animal to animal Doutrelepont obtained only negative results, as also did Senn.

We have already noticed Hanau's successful inoculations of squamous-celled carcinoma of a rat into the abdominal cavities of two other rats; and it remains to mention the transplantation experiments of Dr. Wehr,‡ who successfully transferred a vaginal carcinoma of a dog into the subcutaneous tissue of the belly of another female dog. Valvular apertures in the skin were made in four places and a piece of tumour was pushed into each with antiseptic precautions. The experiment was performed in December, 1887. The nodules increased in size and the animal died in June, 1888, much emaciated. At the autopsy the retroperitoneal glands and the spleen were occupied by secondary growths.

By injecting cancer juice triturated and mixed with distilled water into the jugular vein of dogs, certain results have followed in the hands of Langenbeck, Follin, and Lebert. Nodules have been found in some of the internal organs, but the results are of no value owing to the lack of sufficient histological investigation as to their nature. For it is well-known that inert solid particles if lodging in internal organs excite a local inflammation and production of fibroid tissue, which may attain some size; this is a well-known occurrence in the lung in masons, for instance.

Hahn ('Berlin Klin. Woch.,' 1888) has shown that in the human subject it is possible to transplant a cancerous nodule from one spot to another in the *same* person, with the result of the graft increasing in size and invading the surrounding tissues.

In the 'Progrès Médical,' No. 16 (1889), Darier reports that he has found coccidia in the epithelium in "Paget's Disease of the Nipple." He concludes that not only this disease, but the carcinoma that often follows it, are caused by the parasite; and at a meeting of the Pathological Society (March, 1890), J. Hutchinson, junr., showed specimens which he believed were confirmatory of Darier's observation.§

* "Studien zur Aetiologie des Carcinoms" ('Berlin Klin. Wochenschrift,' 1888).

† "Surgical Relations of Micro-organisms" ('Transactions of the American Surgical Association,' vol. 6, 1888).

‡ 'Transactions of the eighteenth Congress of German Surgeons,' Berlin, 1889, "Weitere Mittheilungen über die positiven Ergebnisse der Carcinomüberimpfungen von Hund auf Hund."

§ Neisser, in the 'Viertelj. f. Derm. u. Syphilis,' 1888, expresses his belief that

Whether the carcinoma which follows in certain cases of Paget disease is caused by these organisms, which presumably produce t cutaneous lesion, remains open for future investigation, as does al the question whether there are in cancerous tumours generally par sites of the same, or of an allied, nature, but which from their sim larity to the cells of the infected tissues have hitherto escaped notic

"On the Position of the Vocal Cords in Quiet Respiratic in Man and on the Reflex-Tonus of their Abductor Muscles By FELIX SEMON, M.D., F.R.C.P., Assistant Physician charge of the Throat Department of St. Thomas's Hospita and Laryngologist to the National Hospital for Epilepsy ar Paralysis, Queen Square. Communicated by Prof. VICTO HORSLEY. F.R.S. Received May 25,—Read June 12, 1890

Although the laryngeal phenomena attending the act of respiratic in man have not escaped the attention of physiologists and laryng logists, yet investigation on this point has been comparative limited and nothing like unanimity of views has been obtained. C the contrary, a perusal of the chapters devoted to the description the mechanism of respiration in the admittedly best and most rece physiological text-books shows that there exists a very remarkat diversity of opinions, not merely on details or on points of seconda importance, but on the very question, whether the larynx plays active *rôle* during quiet respiration in man or not.

Thus Hermann,* Dalton,† Landois, and Stirling‡ describe t glottis in man during quiet respiration as in a condition of rhythmic widening and narrowing; Grützner§ as forming a small triangle n differing considerably from that seen after death, the larynge muscles being in a state of inaction; Rosenthal‖ as being pret widely open, this being due to some muscular action, not precise described; Michael Foster¶ as sometimes in a state of rhythmic widening and narrowing and sometimes in the same state as se after death, this being due to an equilibrium between the dilati

molluscum contagiosum is a disease due to the presence of Psorozoa; and bo Darier and White, of Boston, have described similar Protozoa as being the essent cause of a rare form of skin disease, which has been named "keratosis folliculari or "psorospermosis folliculaire végétante."

* 'Physiologie,' 1870, p. 156.

† 'A Treatise on Human Physiology,' 1867, p. 223.

‡ Hermann's 'Handbuch der Physiologie,' vol. 1, Part ii, p. 57, *et seq.*

§ 'A Text-book of Human Physiology,' 2nd edit., vol. 1, p. 252.

‖ Hermann's 'Handbuch der Physiologie, vol. 4, Part ii, pp. 231, 232.

¶ 'A Text-book of Physiology,' 1889, p. 548, and 1879 (3rd edit.), p. 604.

and constricting muscles; Vierordt* and MacKendrick† as being widely open; and Brücke‡ as being kept open during inspiration.

Three widely different conditions therefore are described as representing the actual laryngeal phenomena observed during quiet respiration in man, viz.:—

(*a*.) Rhythmical opening and narrowing of the glottis.

(*b*.) A condition of rest, the glottis being widely open.

(*c*.) A condition of rest, more or less corresponding to that seen after death.

The first of these descriptions, which dates back to prelaryngoscopic times, is probably the most universally accepted one. It is generally believed that even during quiet respiration, with each inspiration a very perceptible widening, with each expiration a correspondingly perceptible narrowing, of the glottis takes place.

The actual facts, however, little agree with this opinion. From the metric measurements of the glottis, to be described in full further on in this paper, it will be seen that only in a small percentage of cases (less than 20 per cent.) the vocal cords of healthy, quietly breathing adults make rhythmical excursions extending over 4 mm. or more, and that in over 80 per cent. the glottis either remains almost immovably open during both inspiration and expiration, or that the excursions of the vocal cords are hardly appreciable.

The truth of this statement can so easily be verified by laryngoscopic examination that it is not easy to understand how a different belief could so long have held its ground, though a good many explanations of this paradox may be advanced. Thus it may be that some physiologists have simply transferred observations made on animals (in which the rhythmic movements of the cords, as a rule, are much more energetic than in man) to human beings, or that they have drawn their conclusions from an insufficient number of observations on man, or that nervous persons, unaccustomed to laryngoscopic examination, were experimented upon, or that, by the application of the laryngoscope, reflex movements were produced, or finally, that the serious mistake was committed of ordering the persons upon whom the observations were made to "breathe quietly." Under any of the last-named conditions, but especially under the last one, the general type of respiration, in accordance with universally admitted experiences, is apt to change at once in the direction of either deepening or acceleration, and it is under these circumstances that appreciable rhythmic excursions of the vocal cords are seen.

But if a number of healthy, not nervous, adults, accustomed to the application of the laryngoscope, are examined by a skilful observer

* 'Grundriss der Physiologie des Menschen,' 1877, p. 528. See also p. 224.

† 'Text-book of Physiology,' 1889, vol. 2, p. 311.

‡ 'Vorlesungen über Physiologie,' 1881, p. 450.

without their attention being in the least drawn to their mode o respiration, it will be seen, as above stated, that only in a sma fraction appreciable widening and narrowing of the glottis occur and that, in the overwhelming majority of cases, the latter, durin both phases of respiration, forms an almost stationary isosceles tr angle, such as described by Rosenthal. To obtain this result, howeve all the conditions just mentioned must be rigidly adhered to.

Corroborative evidence concerning the almost quiescent state of th glottis in man during quiet respiration will be found in the text-bool and writings of Czermak,* Luschka,† Riegel‡ Schech,§ Vo Bruns,|| Merkel,¶ Mandl,** Tobold,†† Stoerk,‡‡ Bresgen,§ Gottstein,|||| Rosenbach,¶¶ Krause.***

Semeleder,††† Burow,‡‡‡ Prosser James,§§§ and Bosworth|||| merely mention the rhythmic excursions, and B. Fränkel,¶¶¶ thoug he does not directly speak of a state of quiescence, expressly state that even during expiration the glottis is wider open than when see after death. The utterances of Sir Duncan Gibb,**** Türck,††† Fauvel,‡‡‡‡ T. Solis Cohen,§§§§ and Sir Morell Mackenzie|||||| allow of no definite conclusions as to these authors' views on the poir at issue.

* 'Der Kehlkopfspiegel,' 1860, p. 36.

† 'Der Kehlkopf des Menschen,' 1871, p. 49.

‡ Volkmann's 'Sammlung klinischer Vorträge,' No. 95.

§ 'Experimentelle Untersuchungen über die Functionen der Nerven u Muskeln des Kehlkopfs,' 1873, p. 40.

|| 'Laryngoscopie und laryngoscopische Chirurgie,' 1873, p. 101.

¶ 'Anatomie und Physiologie des Stimm- und Sprachorgans,' 1863, p. 120.

** 'Traité pratique des Maladies du Larynx,' 1872, p. 245.

†† 'Laryngoscopie und Kehlkopfkrankheiten,' 1874, p. 126.

‡‡ 'Klinik der Krankheiten des Kehlkopfs,' 1860, p. 68.

§§ 'Pathologie und Therapie der Nasen- Mundrachen- und Kehlkopfkrankheite 1884, p. 34.

|||| 'Die Krankheiten des Kehlkopfs,' 1890, pp. 11, 12.

¶¶ "Zur Lehre von der doppelseitigen totalen Lähmung des Nervus larynge inferior (recurrens)" ('Breslauer aerztliche Zeitschrift,' Januar 24, 1890.—Repri p. 10).

*** "Experimentelle Untersuchungen und Studien über Contracturen der Stim bandmuskeln" ('Virchow's Archiv,' vol. 98, 1884.—Reprint, p. 37).

††† 'Die Laryngoscopie,' 1863, p. 6.

‡‡‡ 'Laryngoskopischer Atlas,' 1877, p. 25.

§§§ 'Sore Throat,' 1878, p. 60.

|||||| 'A Manual of Diseases of the Throat and Nose,' 1881, p. 16.

¶¶¶ In v. Ziemssen's 'Handbuch der spec. Pathol. u. Therap.' 2nd edit., vol. —Reprint, p. 54.

**** 'Diseases of the Throat and Windpipe,' 1864, p. 453.

†††† 'Klinik der Krankheiten des Kehlkopfs,' 1866, p. 80.

‡‡‡‡ 'Traité pratique des Maladies du Larynx,' 1876, p. 79.

§§§§ 'Diseases of the Throat and Nasal Passages,' 1879, pp. 60 and 61.

|||||||| 'A Manual of Diseases of the Throat and Nose,' vol. 1, 1880, p. 242.

The nature of the question evidently renders it impossible to give absolute proof of the quiescence of the glottis in quiet respiration in man, but there cannot be the slightest doubt that repetition (under the necessary precautions) of the observations to be detailed hereafter will fully corroborate the statements concerning this point so far made, and for the purposes of the present investigation they may fairly be taken as proven.

If this then be granted, the question arises whether the quiescent state of the glottis as seen during tranquil respiration is identical with the condition seen after death (the cadaveric position) or whether during both phases of respiration the glottis during life is wider than it is in the dead body. This question is one of fundamental importance for the present investigation. If it were true, as assumed for instance by Grützner and Michael Foster, that the width of the glottis during tranquil breathing is identical with the cadaveric position, the larynx would be reduced, so far as its participation in normal respiration is concerned, to the passive *rôle* of an air-conducting tube, and would thus be put on a par with the trachea and the bronchi. This position of the vocal cords could be expressive of one of two conditions only, namely, either of a state of complete inaction of the vocal cords (Grützner), or what practically amounts to the same, of a state of complete equilibrium between the abducting and adducting forces (Foster). In either case there would be no active participation of the larynx in the function of respiration.

If, on the other hand, Rosenthal's, Vierordt's, and MacKendrick's contention were true, that the glottis is widely open during both phases of quiet respiration, or at any rate wider than after death, it would follow with logical necessity that the state of things seen during life represents neither an equilibrium between the antagonistic adducting and abducting forces, nor a state of inaction of both of them, but that it must necessarily be due to some actual muscular force which would be at work constantly during life. The result of the action of this force being that the glottis is more dilated even during quiet respiration than it is after death, this would obviously seem to signify that its function is to facilitate the act of respiration by allowing a freer ingress and egress of air to and from the lungs than would be possible if the vocal cords were either in a state of inaction or of balance of the antagonistic motor forces. The larynx then would not play a mere subordinate part in respiration as commonly supposed and come into action only as an accessory or associate in case of need, that is, during *forcible* respiration, but would be in a state of *permanent activity* during life, and those of its muscles which keep the glottis wider open during ordinary respiration than it is after death would, of necessity, belong to the class of *regular respiratory muscles* and would *deserve a more prominent position than has been hitherto accorded to them.*

An attempt will be made in this paper to show that the actual conditions correspond to the second of these two alternatives. The question has occupied my attention for a very considerable length of time, and as far back as 1884 I submitted the considerations of which this paper is an outcome to the opinion of the Laryngological Section of the International Medical Congress, at Copenhagen, in the discussion on "An etiological classification of the motor impairments of the larynx."*

On the same occasion Professor Krause, of Berlin, communicated his ideas on the reflex-tonus of the abductors of the vocal cords, ideas which fully harmonise with my own, and which shortly afterwards found full expression in his paper in Virchow's 'Archiv,' above referred to. They are exclusively based upon theoretical considerations.

In order to show that the glottis is wider open during quiet respiration (both inspiration and expiration) than after death, or after division of the vagi or recurrent laryngeal nerves, proofs of a threefold nature may be adduced, namely:—

First. Corroborating evidence from trustworthy observers.

Second. Direct comparative measurements of the width of the glottis during quiet respiration and after death.

Third. Results of experiments on animals.

a. Corroborating Literary Evidence.

In a previous paragraph, the names of those observers have been given who maintain that the glottis during quiet respiration is in a quiescent state. Of these observers, Rosenthal, Vierordt, Czermak, Luschka, Von Bruns, Schech, Riegel, Fränkel, Rosenbach, Krause, Gottstein expressly state that the glottis during both phases of quiet respiration is wider open than after death, whilst the opinion of almost all the other authorities named appears to go to the same effect, but is not so distinctly stated that they could be quoted as partisans of this view. On the other hand, one laryngologist only, namely, Mr. Lennox Browne,† expresses decided views as to the identity of the state of the glottis as seen during quiet respiration and after death.

He figures (Plate 10, fig. 92) the appearance of the normal larynx after death, showing the cadaveric position of the vocal cords, and adds (p. 334), "This is also their position during quiet respiration."

* A short reference to these observations will be found in the Transactions of the Congress ('Compte-rendu des travaux de la Section de Laryngologie,' Copenhagen, 1886.—Reprint, p. 48), but the work upon which they were based is now for the first time published.

† 'The Throat and its Diseases,' 1st edit., 1887.

The weight of evidence, therefore, is entirely on the side of the glottis being wider open during quiet respiration in life than after death.

b. Direct Measurements of the Width of the Glottis during Quiet Respiration in Man.

Surprisingly few direct measurements of the width of the glottis during quiet respiration have apparently been made.

In the childhood of laryngoscopy, graduated mirrors were recommended for making accurate observations concerning this and other points of physiological interest, and some short notice on such mirrors will be found in almost every text-book of laryngology.* On the whole, however, it appears that very little practical use has been made of these mirrors. Altogether, I find only the following metric† statements on the width of the glottis as seen during life and after death:—

Authors.	During Life.			After Death.	
	Position of rest during quiet respiration. Width of glottis.	Margins of respiratory excursions during ordinary respiration.	Width on deep inspiration.	Width of the glottis in Men.	Women.
Luschka‡	8—10	..	..	5—6	3—4
V. Bruns§........	8—10	..	..	..	..
B. Fränkel‖	..	..	..	5—6	3—4
Merkel¶	..	..	..	*ca.* 6	*ca.* 4
Huschke**	..	..	..	*ca.* 4	..
I. Solis Cohen†† ..	..	6—12	12—20	..	..
Morell Mackenzie‡‡	..	6—12	..	..	..

The numbers given in this table, however, for the following reasons, can only be used with great discretion. To begin with, Solis Cohen's and Mackenzie's statements have been added for the

* See, for instance, Merkel, *loc. cit.*, p. 5; Mandl, *loc. cit.*, p. 116; Semeleder, *loc. cit.*, p. 24; Türck, *loc. cit.*, p. 142; Mackenzie, *loc. cit.*, p. 224, &c.

† For convenience, I have reduced all of them to millimetres.

‡ *Loc. cit.*

§ *Loc. cit.*, and, p. 87, *ibid.*

‖ *Loc. cit.*

¶ *Loc. cit.*, p. 172 and 173.

** See Merkel, *loc. cit.*, p. 172.

†† *Loc. cit.*

‡‡ *Loc. cit.*

sake of completeness only, as it will be seen from the table that they give no direct measurements for quiet respiration, which in the present investigation alone is of importance. Moreover, the passages in their works in which the statements in question occur may be differently interpreted.* Secondly, only Luschka and Von Bruns give numbers *comparing* the different positions as seen during life and after death. Thirdly, it does not appear, from the statements of any of the authors quoted, whether their numbers are outcomes of direct *measurements* or of mere *estimates*, and even if the former, upon how many measurements their statements are based. Finally, and this is no doubt the most important objection against drawing definite conclusions from the above table, it is not stated in the writings of any of the authors quoted whether the numbers given by them, be they the outcomes of direct measurements or not, refer to the *actual* width of the glottis or to the *apparent* one, as seen in the graduated mirror.

I will, therefore, draw no inferences whatsoever from this table, and only direct attention to the facts that, with all these shortcomings, (1) the minimum of the numbers given for the position of rest by Luschka and Von Bruns is larger than the maximum of the width after death, as given by all the observers who have expressed this in numbers (Luschka, Von Bruns, Fränkel, Merkel, and Huschke); and that (2), even if we were to take into consideration the rhythmical excursions as given by Solis Cohen and Mackenzie, their minima would just correspond to the stated maxima of the width of the glottis in the dead body.

As I felt, however, that the evidence on this point, for the reasons above given, was by no means conclusive, I began this investigation by making a large number of direct measurements of the width of the glottis in adults during quiet respiration and after death. The number of my observations on living persons amounts to fifty, that on dead bodies to twenty-five.† I need not say that in every instance the measurements were made with great care, strictly in accordance with the rules enumerated in a foregoing paragraph, and that in every case of observation of the living subject the graduated mirror was repeatedly introduced, and the average taken from the numbers gained. It may, however, here be stated that in one and the same person, unless his attention be called to his mode of breathing or

* Solis Cohen says, "The space across will vary ordinarily from three to six lines, but when widely dilated by a deep inspiration it may be from six to ten lines, leaving a space large enough often to admit a good sized finger." Mackenzie says, "On inspiration they (the vocal cords) appear almost to touch each other at their anterior insertion, but to be separated from $\frac{1}{4}$ to $\frac{1}{3}$ an inch posteriorly."

† For some time I examined the larynges of all adults of whom a *post-mortem* examination was made in St. Thomas's Hospital.

extraneous causes influence his respiration, the state of the glottis during tranquil breathing remains pretty constant, as I have ascertained by measuring several persons on different days, and by comparing the results thus obtained.

As to the method, I availed myself of a mirror on which a millimetre scale is engraved in such a direction that when the mirror is held in the correct position the scale stands parallel with the plane to be examined, that is to say, the distance between the inner borders of the arytenoid cartilages. The source of confusion which this arrangement undoubtedly entails, viz., that the marks of division of the scale are themselves reflected in the mirror if the latter be held in the correct position, that is to say, at an angle of 45° towards the horizon, is easily eliminated by a little practice.

There is, however, another point of the greatest importance with regard to the exactitude of the measurements, and one which, with the exception of a passing remark of Mandl's (*loc. cit.*, p. 116), I find to my surprise is not mentioned by any previous observers who speak about the use of these graduated mirrors; I refer to the considerable difference of the *actual* from the *apparent* length of the distance measured. The distance between the inner surfaces of the arytenoid cartilages, as apparent on the scale engraved on the laryngeal mirror, is not equal to the real distance, but considerably smaller than this, and the proportion between the real and the apparent lengths is, according to the principles of physiological optics, the same as the proportion between the real distance of the object, on the one hand, and the distance of the mirror, on the other, from the observer's eye respectively.

The real distance of the object (*i.e.*, the level of the glottis) from the observer's eye is of course equal to the distance of the observer's eye to the centre of the mirror plus the distance of the centre of the mirror to the object.

If we therefore assume, with B. Fränkel (*loc. cit.*, p. 16), that the average distance of the observer's eye from the centre of the mirror is 22 cm. (of which 14 cm. go to the distance to the mouth of the person examined, and 8 cm. to the distance from the mouth to the mirror), and that in an adult man of middle size the average distance from the centre of the mirror to the glottis is 8 cm. (in an adult woman 6 cm.) we obtain, if we call the apparent lengths of the base of the glottic triangle as seen during quiet respiration in the mirror a, and the real length which is wanted x, the following proportions *:—

* The same result is obtained if the ratio of the size of the scale on the mirror to the real size of the object be enquired into. If the distance from the eye to the mirror = A, and the distance from the mirror to the object = B, the whole distance from eye to object = A + B; the apparent size of the object will be to that of the scale on the mirror as $\frac{A}{A + B}$.

In man : $22 : 30 = a : x$.
In woman : $22 : 28 = a : x$.

Whence it follows that in men : x is equal to $\frac{30a}{22}$ or $= a\ 1{\cdot}36$. In women: $x = \frac{28a}{22} = a\ 1{\cdot}27$.

It is obvious that in spite of all these precautions no absolutely correct numbers can be obtained, because, on the one hand, the distance of the observer's eye, even if he be emmetropic, is not always exactly equal to 22 cm. from the centre of the mirror, and, on the other, the distances of the level of the glottis from the centre of the mirror as given above for men and women are only *average* figures, from which the exact figures in individual cases might not inconsiderably differ. Still I have found by measuring in a double manner a large number of dead larynges in order to control my laryngoscopic measurements (viz., by first examining them with the graduated mirror, whilst the conditions of life were as carefully as possible imitated so far as position, distances, &c., are concerned, and by afterwards introducing a fine compass into the larynx, and measuring directly the distances between the inner surfaces of the arytenoid cartilages), that with sufficient practice nearly accurate results are obtained by direct measurements. The maximum error committed in measuring laryngoscopically was 1 mm., the average ½ mm., while in a large number of cases the laryngoscopic and direct measurements completely corresponded with one another.

Of the measurements of dead larynges, it has already been stated

Thus if $A = 22$
and $B = 8$

$A + B = 30 =$ the distance (if the person examined be an adult man).

The ratio of size of scale to the object will then be $= \frac{22}{30} = 0{\cdot}733$; *i.e.*, 0·733 mm. on the mirror scale correspond to 1 mm. real size.

If the eye be removed to a small extent further from the mirror, say to 24 cm. instead of 22 cm., the result will not be very different, as both the numerator and denominator will be increased. In the first case, as before stated,

$$\frac{22}{30} = 0{\cdot}733.$$

In the latter

$$\frac{24}{32} = 0{\cdot}75.$$

The real size (x) will then be to the apparent (a) $= 1 : 0{\cdot}733$, or $x = \frac{a}{0{\cdot}733}$.

This gives the real width of the glottis in adult men.

In women $x = \frac{a}{0{\cdot}75}$.

I am much obliged to Mr. Stevenson for having given me this formula.

that a large number of them were examined by both laryngoscopic and direct methods; the remainder were measured directly. It need hardly be added that all undue traction, pressure, or, in short, anything that could possibly tend to disturb the natural condition of the parts was carefully avoided. To obviate the objection that the natural condition might have been disturbed by the mere removal of the larynx from the body, prior to the distance in question having been ascertained, I have made some laryngoscopic measurements on dead bodies before the *post-mortem* was commenced,* and have compared the results thus obtained with the laryngoscopic and direct measurements taken afterwards when the larynx had been removed from the body. It was found that the width of the glottis had not been altered by the removal of the larynx.

I now proceed to quote the results I obtained.

In a total number of 50 persons whom I have methodically examined with the graduated mirror during quiet respiration, I have only found regular and considerable rhythmical movements eight times. Considerable movements I call all such, in which the excursions of the cords influence the width of the glottis during the two phases of respiration to an extent of more than 4 mm. Of the remaining 42 persons examined, 23 were men, 19 women. In all these cases during quiet respiration the glottis remained either immovably open, or the excursions of the cords were so slight as to easily allow of estimating the average width of the glottis. Subjoined are the results of my experiments, expressed in millimetres, both the apparent and the real size being given in round numbers.

* I may state here, that, according to repeated examinations of my own, rigor mortis in man exercises no influence upon the width of the cadaveric glottis.

Males.			Females.		
Age.	Average size of the glottis.		Age.	Average size of the glottis.	
	Apparent.	Real.		Apparent.	Real.
32	8	11	19	8	10
36	9	12	26	8	10
28	9	12	34	10	12·5
22	10	13·5	33	8	10
34	9	12	28	9	11·5
55	7	9·5	60	7	9
46	9	12	43	8	10
53	10	13·5	25	10	12·5
48	10	13·5	44	9	11·5
26	12	16	53	8	10
27	10	13·5	19	8	10
30	9	12	22	7	9
44	8	11	22	10	12·5
28	10	13·5	34	11	14
20	7	9·5	21	10	12·5
53	11	15	45	9	11·5
59	10	13·5	37	12	15
45	9	12	26	10	12·5
19	10	13·5	21	9	11·5
23	14	19			
58	9	12			
22	8	11			
60	11	16			

From these tables it will appear that my own measurements on the whole agree, so far as the apparent distance is concerned, with those of Luschka and Von Bruns, who alone have expressed these conditions in numbers. I can only assume that both these authors speak of the apparent, not of the real, size when stating that the average width of the glottis in both cases varies from 8 to 10 mm.; this, it is seen, is also the average of the apparent size in my own measurements, the maximum being in men 14,* in women 12 mm., and the minimum in both cases being 7 mm. The exact average as resulting from my observations would be in men an apparent width of nearly 10 mm., corresponding to an actual width of 13·5 mm., and in women an apparent width of exactly 9 mm., corresponding to an actual width of 11·5 mm. I cannot say that I have been able to make out any distinct relationship between the widths of the glottis and the ages or statures of the persons examined.

* This figure, corresponding to 19 mm. real size, entirely confirms the apparently somewhat exaggerated descriptions given by Czermak and Solis Cohen, who state that the glottis in some cases, even during quiet respiration, would admit a good-sized finger.

The following are my results as to the width of the cadaveric glottis:—

Men.		Women.	
Age.	Real size of cadaveric glottis.	Age.	Real size of cadaveric glottis.
35	5·5	21	6
17	4	40	2
24	5	27	4
23	5	56	2·5
48	4·5	54	4·5
50	5	58	3·5
54	6	33	4
63	4	23	4·5
58	3·5	60	5
26	4		
45	5		
27	3·5		
60	5·5		
77	4·5		
35	5		
48	5		

Here again on the whole, my results agree with those found by previous observers (Luschka, Von Bruns, B. Fränkel, Merkel, Huschke). In 16 larynges of adult males I found a maximum width of 6, a minimum of 3·5 mm., whilst the average was very nearly 5 mm.; in 9 larynges of adult females the maximum width was 6, the minimum 2 mm., the average exactly 4 mm.

It can be positively stated that no relationship exists between the cadaveric widths of the glottis and the ages or statures of the bodies, since the maximum width seen in the female (6 mm.) was observed in a small woman, who had suffered from tuberculosis of the lungs; the minimum width seen in a man (3·5 mm.) was observed in a tall lad of 19 who died from renal disease.

At the same time I must not omit to observe that the remarkable differences in the width of the cadaveric glottis as shown by the last table, especially in females (from 2 to 6 mm.) make it to my mind rather doubtful whether the expression "cadaveric position of the vocal cords" is a very significant and useful one.

In an isosceles triangle of 20 mm. length, this being the average length of the glottis in females, it makes a very considerable difference in the position of the sides whether the glottis be 2 mm. or 6 mm. in length.*

* The last-named fact appears to be of considerable importance, with regard to

If we now compare the summary of the two last tables showing the width of the glottis during quiet respiration and after death respectively, the results will be found to be rather surprising.

The width of the glottis in adults expressed in millimetres is:—

In men.		Average.	Maximum.	Minimum.
During quiet respiration	apparent size	10	14	7
	real size	13·5	19	9·5
After death		5	6	3·5

In women.		Average.	Maximum.	Minimum.
During quiet respiration	apparent size	9	12	7
	real size	11·5	15	9
After death		4	6	2

In other words, first, *during quiet respiration the width of the glottis in both sexes* is on the average not only fully twice the size or more of the glottis as seen after death, as would appear from mere laryngoscopic estimation, but *in reality twice and a half to nearly three times that size.*

Secondly, if, instead of the averages, the maxima and minima respectively be compared, the differences in some instances are even larger.

Thirdly, under all circumstances the minima as observed during life are greater than the maxima seen after death.

Thus all metric observations go to prove beyond any doubt that the glottis during quiet respiration is much wider open than after death.

Thirdly.—Experiments on Animals.

Concerning the last category of proofs, to the effect that the glottis is wider open during quiet respiration than after death, namely, experiments on animals, I am able to state that from the first experimenter—Legallois, down to the present day, all physiologists who have performed division of either both pneumogastric or both recurrent nerves on animals, and whose works on the subject have been

the different descriptions by authors, as to what may be called paralysis of the recurrent laryngeal nerve and what abductor paralysis.

accessible to me in the original, namely, Legallois,* Reid,† Longet,‡ Traube,§ Rosenthal,|| Dalton,¶ Georg Schmidt,** Schech,†† Steiner,‡‡ Vierordt, jun.,§§ unanimously state that after such division the glottis became narrower than it had been previously; and this in fact is a statement which will be found in every text-book of physiology.

This category of proofs, however, obviously again can only be used with great restrictions for the decision of the point at issue. In the first place, of the authors just named, only Legallois, Reid, Traube, Schmidt, and Schech state distinctly and unmistakably that the narrowing of the glottis of which they speak as a sequel to the division of the pneumogastric and recurrent laryngeal nerves refers to the difference of the position of the vocal cords thus obtained from the one previously seen during *quiet* respiration. It can hardly be doubted that, also, the other authors quoted above think of this difference when they speak of the narrowing resulting from the division; but, unfortunately, their statements on this important point are not so unequivocal as to altogether exclude the objection that they had intended to contrast the position resulting from the division of the motor laryngeal nerves with that observed during *deep inspiration.*

Secondly, the conditions of quiet respiration in men and animals, so far as my own observations during a long-continued series of experiments on the functions of the motor laryngeal nerves and on the central innervation of the larynx undertaken in conjunction with Professor Victor Horsley permit me to conclude, are so different from each other, in that the quiet respiration of animals is much more usually accompanied by rhythmical excursions of the vocal cords than in men, that too much stress must not be laid on experiments in animals with regard to this point.

Thirdly, there exist undoubtedly frequent and important anatomical

* 'Expériences sur la principe de la Vie,' 1812, p. 197; and 'Œuvres,' 1830, vol. 1, pp. 170, *et seq.* and p. 248.

† 'Physiol., Anatom. and Pathol. Researches,' 1848, p. 118. His paper on this subject was published in 1841.

‡ 'Gazette Médicale de Paris,' 1841, p. 469, and 'Traité de Physiologie,' vol. 3, p. 529.

§ 'Beiträge zur experimentellen Pathologie und Physiologie,' 1846, fasc. 1, p. 95, *et seq.*

|| 'Die Athembewegungen u. ihre Beziehungen zum Nervus Vagus,' 1862, 77.

¶ 'A Treatise on Human Physiology,' 1867, p. 451.

** 'Die Laryngoscopie an Thieren,' 1873, p. 31, *et seq.*

†† 'Experimentelle Untersuchungen über die Functionen der Nerven u. Muskeln des Kehlkopfs,' 1873, p 31.

‡‡ 'Die Laryngoscopie der Thiere.' Reprint from 'Verhandlungen des Natur. hist. Med. Vereins zu Heidelberg,' N.S., vol. 11, Heft 4, p. 287.

§§ 'Beiträge zur experiment. Laryngoscopie.' Diss. inaug., 1876, p. 39.

variations of nerve supply, not only in different kinds of animals, but even in animals belonging to the same species; and again, as will hereafter be shown, the immediate effects of the division of the motor nerves of the larynx, as well as its ultimate consequences, are different according to the age of the animals used for the experiments.

Thus, unless care is taken, all these circumstances combine to reduce, in this particular question, the value of experiments on animals for the solution of the corresponding question in men. Still, even if regard be had to all these circumstances and possible sources of error, the fact remains that by *all* observers narrowing of the glottis in all kinds of animals has been reported after division of the motor nerves of the larynx, and that by some of these observers the state of things thus resulting is expressly contrasted with the position as previously present during quiet respiration.

A general survey of the mass of evidence so far accumulated proves beyond doubt that the glottis is wider open during quiet respiration (inspiration and expiration) than it is after death or after division of the pneumogastric and recurrent laryngeal nerves. The statements of the most trustworthy and experienced observers on men, the reports of all physiologists who have investigated this question by experiments on animals, and especially the direct comparative measurements of the glottides of quiet-breathing healthy adults and of dead adult bodies—all go to establish one and the same result, and leave, I think, no doubt as to the actuality of the fact that the glottis in man during quiet respiration is wider open than after death.

The immediate outcome of this result is, as already stated in a previous paragraph, that the position of the vocal cords during quiet respiration can neither represent an equilibrium between the antagonistic adductor and abductor muscles of the larynx, nor the result of inaction of both of them. In either of these two hypothetical conditions the width of the glottis during quiet respiration could not but be identical with that seen after death.

As matters actually stand, the state of the glottis during quiet respiration must necessarily be the result of *active muscular contraction*, and must represent one of two conditions, viz., either simultaneous activity of both the adductors and the abductors of the vocal cords with preponderance of the latter, or, secondly, some degree of activity on the part of the latter alone—the adductors being not at all in a state of functional activity. An attempt will be made hereafter to show that the second of these alternatives in all probability corresponds with the actual facts. But before proceeding to a discussion of this point, the question which naturally arises from the fact that the glottis is wider during quiet respiration than after death demands a reply: What is the *cause* of this difference?

A reply to this question will be given by a consideration of the physiological functions of the larynx.

The larynx serves two functions, which are in a certain sense intimately connected with, yet in another sense just as distinctly antagonistic to, each other. These are the functions of respiration and of phonation. For the purpose of the former—which with regard to the vital interests of the individual is by far the more important one of the two—it is indispensable that the lumen of the air tubes should be wide enough open to admit of the ingress and egress of the quantity of air necessary for breathing purposes, without at the same time imposing an additional labour upon the other respiratory muscles. Such an additional labour would arise if the portal for the entry and exit of air were so narrow that the air, instead of quietly passing, had, by forcible means, to be sucked through it.

On the other hand, the function of phonation makes it a necessity that an apparatus should be interpolated within the air tubes which would admit of complete juxtaposition of the voice-producing organs. This interpolation, in all probability, was meant for the purposes of phonation only, not primarily for respiratory purposes.

I am perfectly well aware that in a certain sense the interpolation of the phonatory apparatus, represented by the vocal cords, subserves also the protection of the lower respiratory passages against the entry of foreign bodies: but that this interpolation is not indispensable is conclusively shown by comparative anatomy. The purpose of protection is, indeed, as demonstrated by the latter science, sufficiently provided for by the "constrictor vestibuli laryngis" (Luschka) or "thyreo-ary-epiglotticus" muscle (Henle), which forms the uppermost stratum of the sphincter muscles of the larynx. In Reptiles a sphincter of the simplest form surrounding the vestibule of the larynx is the only protective arrangement (Henle, 'Anatomie,' vol. 2, p. 249), and even in dumb *Mammalia* the same simple arrangement returns. "In the Cetaceous Mammalia," says Mayo ('Outlines of Human Physiology,' 1839, p. 380), "which are dumb, we find a respiratory larynx alone; the windpipe terminates in a contractile circular aperture, and this opens not at the root of the tongue, but is prolonged as a pipe towards the nostrils completely out of the way of food." In man, again, a contraction of the constrictor vestibuli of the larynx sufficiently guards the lower air passages against the entry of foreign bodies, as shown by the numerous cases in which, though destruction of the phonatory apparatus (vocal cords) had taken place through disease, yet accidents from food, &c., "going the wrong way" occur no more frequently, than in people in whom these parts are intact.

Thus comparative anatomy as well as pathological observation on men combine to show that the interpolation of the vocal cords is by

no means an indispensable adjunct to the purposes of the respiratory process.*

On the other hand, in order to impart vibrations to the column of air contained in the upper respiratory passages for the purpose of producing sound, nature chose as the most suitable form a reduplication of folds of mucous membrane within the larynx endowed with certain special characteristics.

But this reduplication in turn seriously interferes with the calibre of the tube; that is to say, with the respiratory function of the larynx.

That this interference is serious can easily be shown anatomically, and, though not quite so obviously, with the material at present in our possession, both physiological and pathological.

For the first purpose I have made the following measurements† of dead larynges :—

Age of subject.	Transverse diameter of vestibule of larynx.	Width of glottic base.	Total length of glottis.	Longitudinal diameter of cricoid cartilage immediately beneath vocal cords.	Transverse diameter of cricoid cartilage immediately beneath vocal cords.
(*a.*) Males—					
23	18	5	25	15	16
50	16	5	24	16	16
43	17	4·5	24	16·5	17
54	17	6	22	18	19
63	17·5	4	23	18·5	18·5
(*b.*) Females—					
33	13	4	20	13	13
23	13	4·5	20	13	14
60	14	5	21	13·5	14

From this table it appears that the transverse diameter of the vestibule of the larynx, on the one hand, and the longitudinal and the transverse diameters of the cricoid cartilage, on the other, are so nearly equal to each other, that in both sexes, without committing

* At the same time it is perfectly possible that in *some* species of animals the complete closure of the glottis is by far the most important means of protecting the lower air passages against the entry of food. With regard to this very complicated question, see the excellent prize essay of Otto Frey, 'Die pathologischen Lungenveränderungen nach Lähmung der Nervi Vagi,' 1877, p. 81.

† My numbers referring to the total length of the glottis in men differ somewhat from those of previous observers in that they are a little smaller; the accuracy of the observations, however, can be vouched for.

any considerable error, the space above and below the vocal cords and ventricular bands might be looked upon as a circle. Selecting now the average radius resulting from the comparison of my measurements as the representative of the circle, we find in the five measurements of male larynges that its average would be about 8 mm. The area, therefore, in adult men of the air-tube above and below the phonatory apparatus would be about 200 square mm. In women the average radius would be 6·5 mm.; the area of the air-tube above and below the phonatory apparatus would be therefore about 133 mm.

Now, the average length of the glottic triangle in men after death is a little more than 23 mm., its base, as shown before, is 5 mm.; the area of the space included by the sides of the glottic triangle in men after death, therefore, is about 57 mm.

In women, the average length of the glottis would be 20 mm., its width, as shown before, is 4 mm.; the area of the glottic space, therefore, 40 mm. This means, in other words, that in adult individuals of both sexes the space for the entry of air is reduced by the interpolation of the phonatory apparatus when its constituent parts are at perfect rest to *less than one-third of its natural area.*

Anatomically, therefore, there can be no question as to the gravity of the diminution of the air channel, and the more so because it must not be forgotten that, even apart from the interpolation of the phonatory apparatus, the larynx and trachea represent the narrowest part of the whole air passages, both the uppermost part of the air passages and the aggregated diameter of the bronchi being considerably larger than the calibre of the first-named parts.

Physiologically, the question immediately arises: Is so considerable a diminution of the lumen of the air passages as that produced by the interpolation of the phonatory apparatus when at complete rest compatible with what we call at present the normal type of quiet respiration?

In this sentence the expression "normal type of quiet respiration" demands further explanation. It is by no means the purpose of this paper to show that, even if the glottis during life was identical with that seen after death, necessarily that state of laboured respiration which we call "dyspnœa" would arise. Obviously, if the interference caused by the diminution of the calibre be not excessive, a condition of things could be imagined in which this diminution was counterbalanced merely by what is called "hyperpnœa," *i.e.*, either an increased labour of the muscles normally engaged in quiet respiration (diaphragm, intercostal muscles, and scaleni), or additional efforts of the so-called "accessory" muscles of respiration. Under such circumstances, though one could hardly speak of the presence of actual dyspnœa, yet, undoubtedly, the state of things thus created would be different from what is at present the general idea of quiet

respiration in man. It will at once be seen that it is necessary to define this point very clearly, because, as already mentioned before, this part of the investigation is beset with very considerable difficulties.

Whilst, up to this point, the main part of our argument has been based on physiological observations on the human subject, and clinical facts and experiments on animals were only used as corroborative evidence, in this part of our enquiry we have to depend exclusively upon the two last-named factors, and for various reasons neither of them gives us so distinct a reply to the question at issue as would be desirable.

First, with regard to experiments on animals.

It has already been stated in a previous chapter that the conditions resulting from section of the laryngeal motor nerves in animals vary very considerably according to species, age, and individual peculiarities of the animals experimented upon, and that, practically, the only point on which a complete consensus of opinion has been obtained consists in the fact that narrowing of the glottis has been described by all observers. To what degree, however, this narrowing interferes with the respiratory functions of the animal operated upon is a question the replies to which vary very considerably.

The importance of the difference of species and of ages of the animals experimented upon for the decision of this point did not escape the acute observer who first thoroughly studied the influence of the division of the recurrent laryngeal nerves upon life, viz., Legallois. Having observed that in very young dogs the division of these two nerves led to speedy death by asphyxia, he wished to know whether the same phenomena were present in other specimens of animals. He therefore cut, he tells us,* sometimes the pneumogastric, sometimes the recurrent laryngeal, nerves in cats, rabbits, and guinea-pigs, in the first days of their existence. He found that "cats died in the same manner, and perhaps even more quickly than dogs; that in guinea-pigs and in rabbits, section of the recurrents obstructs the glottis less completely, the former only die in about an hour's time, and the rabbits after a half hour."

Having thus stated the differences in degree depending upon the *species* of the animals, Legallois proceeded towards determining the influence of *age* upon the phenomena resulting from sudden diminution of the calibre of the glottis. He found that "section of the recurrent nerves produces a less considerable suffocation in inverse proportion to the age of the animals; thus, in dogs and cats about two to three weeks old, this operation still causes dyspnœa, which, though less strong than in the first days after birth, yet is strong enough to cause the death of the animals after a few hours. At the

* 'Expériences sur le Principe de la Vie,' 1812, pp. 190, *et seq.*

age of three months or more, dogs are no longer so inconvenienced as to die; cats are much more so, and as soon as one excites them and forces them to walk they fall down as though they were suffocated."

Similar conditions obtain in rabbits and guinea-pigs. "The dyspnœa which is caused in their case by division of the recurrent nerves is less grave in proportion to their ages, but it is always more severe in guinea-pigs than in rabbits. For instance, the latter are much less inconvenienced by it at the age of one month than guinea-pigs at the age of five months, which may still perish from it within twenty-four hours."

"The reason of all these differences," continues Legallois, "is easily understood. It consists in the fact that, in proportion to the capacity of the larynx, the opening of the glottis in animals of the same age is greater in one species than in another, and still greater in the adult than at the moment of birth in those of the same species, as already stated by M. Richerand in the human species.* Or assuming that the form of the glottis, on the whole, is similar in these diverse animals, inasmuch as the areas of the smaller figures are to each other as the squares of the homologous dimensions, it is seen that a narrowing of the same kind of the glottic opening must intercept the passage of air in very different degrees."

And Legallois† sums up his remarks as follows: "The diminution of the glottic opening varies according to the species, and much more even according to the age in certain species. In dogs, and especially in cats, it is so considerable that these animals are suffocated as quickly, or nearly so, as if one had ligatured their trachea. In proportion to the growing up of these animals the danger becomes less pressing, and when they have arrived at a certain age they are only slightly inconvenienced by it (namely, by the diminution of the glottis); this, at least, is so in dogs. From this it follows that of all the symptoms which are produced by section of the par vagum, the gravest ones, those that kill most quickly, are in certain cases those which depend upon the larynx. On the whole, whenever the difficulty of breathing becomes very severe immediately after this operation, it is very likely that its principal cause is in the larynx; for instance, the violence with which the dyspnœa declares itself suddenly in horses, even in adult ones, and the promptitude of their death show that in these animals the glottis is considerably narrowed. A large opening made in the trachea furnishes simultaneously both the remedy for and the etiology of all these cases. *The chink of the glottis, therefore, is never the same in the living subject as is found in the cadaver, and the arytenoid cartilages need being supported by their*

* 'Nouveaux Éléments de Physiologie,' 2 edit., vol. 11, p. 436.

† *Ibid.*, p. 231, *et seq.*

muscles just as the upper eyelid needs the support of its own." (The italics are my own.)

So far Legallois. I have quoted him at length, first, because the results of his experiments have, in the main, been corroborated by practically every observer who has repeated his experiments; and, secondly, because it appeared to me most interesting that the same thesis, the establishment of which is attempted in this paper, should have been given out with almost prophetic foresight at the beginning of the century by practically the first worker in this field.

There is no need to quote at any length the experiences of subsequent experimenters, as they agree on all the main points with Legallois' results, and as the whole literature of the subject has been most carefully quoted and abstracted by Frey in the excellent prize essay already mentioned. The general result of all these experiments may fairly be thus summarised:—that the effects of the sudden reduction of the glottis to its cadaveric width vary very considerably, first, with regard to the species; secondly, with regard to the age of the animal experimented upon; thirdly, though in a less degree, with regard to individual peculiarities of the animal. Whilst certain species, such as cats and horses, not only in the first days after birth but even when adult, are suffocated by the reduction of the glottis to the cadaveric size, other species, notably dogs, suffer less and less in proportion to their ages, so that, whilst they die when operated upon a few days after birth, dyspnœa only occurs on exertion when they are operated upon when adult. Again other species, such as rabbits and guinea-pigs, are not nearly so much inconvenienced as the species so far mentioned, even when operated upon at a very early period of their existence.

Thus the general effect, in animals, of reduction of the glottis to the cadaveric size during life is undoubtedly interference with respiration, but the *degree* of interference immensely varies. Obviously, under these circumstances, it is not permissible to draw hard and fast conclusions from experiments on animals with regard to the degree of interference which may be expected under similar conditions in man, and the only reasonable conclusion which can be drawn is that in all probability reduction of the human glottis to the cadaveric position would also lead to *some* interference with normal respiration, and more so in the young human subject than in the adult.

Even less satisfactory than experiments on animals are pathological observations on man for the decision of the question whether reduction of the glottis to the cadaveric width interferes with normal respiration. It is perfectly true that nearly all observers* who have

* For instance, v. Ziemssen, 'Handbuch der speciellen Pathologie,' vol. 4, pt. 1, p. 456; Morell Mackenzie, *loc. cit.*, p. 440; Gottstein, *loc. cit.*, p. 259, &c.

described cases of bilateral paralysis of the recurrent laryngeal nerves (which are extremely rare) agree that there is no dyspnœa when the patients are at rest; but then two circumstances combine to render the value of this statement rather doubtful for the decision of the point here at issue.

In the first place, it is more than likely that only those *coarser* differences of respiration which are termed "eupnœa" and "dyspnœa" respectively have met with attention on the part of clinical observers, and that, as no actual dyspnœa in the clinical sense of the term was met with in such patients when at rest, *finer* differences in the type of respiration, such as intensification or acceleration of respiratory movements on very slight exertion, were not particularly studied. Moreover, some of these observers, as for instance Solis Cohen,* indeed, speak of "moderate dyspnœa" on exertion occurring sometimes under such circumstances.

Secondly, however, a very important element, not mentioned so far, here comes into consideration, namely, the wonderful adaptability of the human organism to very considerable changes in the respiratory conditions, *provided that these changes are produced slowly*. It is an every-day observation with laryngologists, that an *acute* stenosis of the larynx, such as produced, for instance, by acute œdema, interferes, even if by no means very considerably, yet in a much higher degree, with respiration, and produces much greater subjective and objective dyspnœa, than a much higher degree of stenosis due to *chronic* affections, such as growths in the larynx, bilateral paralysis of the glottis openers, cicatrices after ulcerative disease, congenital membranes expanded between the vocal cords, &c.

Now in almost all cases in which bilateral paralysis of the recurrent laryngeal nerves (*i.e.*, the pathological equivalent during life to the cadaveric position of the glottis after death) is produced, the course of events is a very slow one, and the patients have ample time to adapt their entire respiratory mechanism to the altered conditions of the larynx. Under such circumstances, their whole mode of respiration is instinctively changed to such a degree, that the effects of the reduction of the glottis to the cadaveric size are not likely to attract prominent attention.

Yet there can be no doubt in my opinion that in cases of reduction of the glottis to the cadaveric size, except when the act of respiration is at its lowest physiological ebb, *i.e.*, during *complete* rest of the body, a modification of the mechanism of respiration does occur as soon as any demand is made upon the respiratory apparatus.

This opinion is not purely theoretical.

I have never had the opportunity of observing a case of quite complete bilateral paralysis of the recurrent laryngeal nerves, but I have

* *Loc. cit.*, p. 144.

been able for a long time to follow a case in which quite analogous conditions had been produced by perichondritis of the larynx ending in anchylosis of the crico-arytenoid articulations, the cords being fixed in the cadaveric position. The width of the glottic base, measured with the graduated mirror, completely corresponded with about the *maximum* of that seen after death in that it was between 5 and 6 mm. (the patient was a woman). There were no other laryngeal lesions interfering with the calibre of that organ, nor any other affections causing diminution in the calibre of the air passages. The patient when at *complete* rest breathed quietly and without any effort; the number of her respirations per minute was on the average 20. As soon, however, as she was told to twice pace up and down the length of the room in which she was examined, and then to sit down again, the number of her respirations at first very considerably increased, viz., to 36 or 40, then gradually the frequency diminished, but the inspirations became much more intense, the contraction of the scaleni being distinctly visible, and the levatores alæ nasi perceptibly acting. It always took some time until the previous quiet type of respiration was re-established. The patient complained of *considerable* dyspnœa on however slight exertion.

Thinking that a more positive reply to the question might be obtained by a study of the effects in man of *sudden* diminution of the calibre of the glottis to the cadaveric width, such as are unintentionally produced sometimes by section of the recurrent laryngeal nerves during operations for the removal of goîtres, I addressed in 1884 a collective question to several colleagues* as to the respiratory phenomena after sudden bilateral section, of paralysis of the recurrent laryngeal nerves, and of even unilateral paralysis in children. The latter question was inspired by the wish to learn whether, analogous to the conditions observed in young animals, even a minor degree of diminution in the young human subject might lead to considerable respiratory disturbance, a very interesting case reported by Sommerbrodt† having shown that unilateral paralysis of an abductor muscle of the vocal cord (which in the adult so far as respiration is concerned is perfectly harmless) suffices in a child of 1½ year to produce most grave dyspnœa necessitating tracheotomy.

I regret to say that this question has not been productive of any satisfactory reply.

In a paper published since by Jankowski,‡ some statements occur which are of interest in connexion with the subject of the present investigation, inasmuch as they refer to cases in which both recurrent

* 'Internationales Centralblatt für Laryngologie,' &c., 1884, p. 40.

† 'Breslauer Aerztliche Zeitschrift,' No. 10, 1881.

‡ "Lähmungen der Kehlkopfmuskeln nach Kropfexstirpation" ('Deutsche Zeitschrift f. Chirurgie,' vol 12).

laryngeal nerves appear to have been damaged during the operation. Unfortunately, however, the reports are in part incomplete, and moreover the alterations in the calibre of the trachea due to the previous direct compression by the constricting goître also appear to have in many of them played a considerable part in the production of the respiratory phenomena observed after the operation. Thus these cases are by no means pure, and can hardly be made use of for a decision of the question at issue. The most important of them perhaps is one reported by Riedel,* in which, either due to inundation of the wound with carbolic acid solution, or, as would seem more probable, to tearing of the recurrent laryngeals in the course of the operation, within two hours from its end dyspnœa developed. Still even in this case evidence is not pure, as one of the pneumogastric nerves was simultaneously damaged, and the dyspnœa may have been in part referable to this cause.

Thus pathological observation on the human subject so far offers a much less complete reply to the question concerning the effect of the reduction of the glottis to the cadaveric width than might be theoretically expected, and this point will certainly demand continued attention.

All that can at present be fairly said, is that the evidence points in the direction that reduction of the glottis to the cadaveric size involves, upon the commencement of any effort, however small, some alteration in the type of respiration.

Although the evidence concerning the effects upon the respiration in man of the interpolation of the phonatory apparatus leaves a good deal to be desired, as shown in the last two chapters, there can be no doubt, I think, that its whole tenour goes to show, as would, indeed, be expected from the anatomical facts above demonstrated, that this interposition presents a considerable hindrance to the function of quiet respiration, and that for the fulfilment of the latter function it had to be counterbalanced or neutralised to a certain degree.

This neutralisation could evidently have been effected in one of two forms, namely, either in the form of a rhythmical widening and closure of the glottis, such as commonly is supposed to exist even during tranquil respiration, or in the form of a *tonic dilatation* of the glottis during both phases of quiet respiration (inspiration and expiration), supplying the minimum of space compatible with the ingress and egress of that amount of air to the lower air passages which is required for the purposes of what we call normal quiet respiration.

It has been shown in the preceding chapters that both these alternatives are actually met with in the quiet respiration of men, and it is hardly necessary to say that they do not in the least exclude

* 'Centralblatt für die med. Wiss.,' 1882.

one another. On the contrary, the tonic opening of the glottis may and does at any moment under the influence of emotion, mechanical effort, will, reflex irritation, &c., give way to a rhythmical widening and narrowing of the glottis, and this again on return of normal conditions gives way to the tonic state of widening above described. There can, however, be no doubt that this tonic condition is, much more than rhythmic movements, representative of the participation of the larynx in quiet respiration of *man*. This has been amply demonstrated by the mutual proportions described in a previous paragraph.

The question now arises: What does this tonic widening represent, and how is it produced?

It has already been mentioned that it may represent as well a state of tonic innervation of *both* the glottis openers and glottis closers, with preponderance of the former, or, secondly, a tonic innervation of the abductor muscles alone.

The existence of the first-named condition has been very ingeniously argued for by Rosenbach in the paper referred to above. He writes as follows: "The more complete and delicate the innervating mechanism and the active muscular apparatus are, the more exact must, no doubt, be the co-operation of the muscular groups in question. Now, the action of the vocal cords during respiration and phonation requires an extraordinarily delicate mechanism, and the position of the arytenoid cartilages depends in a high degree on the co-operation of all muscles attached to them. Nay, disturbances would have to be registered far more frequently than is now the case if an extensive vicarious action of healthy muscles were not possible, replacing those disabled from fulfilling their functions. It is, therefore, probable that during the normal position of the vocal cords nervous impulses are constantly carried down to both adductors and abductors, but that the abductors, being the stronger muscles, preponderate, just as in other muscular territories even during quiescence the activity of the extensors preponderates. The stronger the inspiratory innervation is, the more will the glottis become opened, and the expiratory constriction, so trifling during quiet respiration, is very likely to be considered mostly as a remission of the innervation of the abductors, *i.e.*, as a more passive occurrence."

In spite, however, of this clever advocacy of a preponderance of the abductors over the adductors, both being supposed to act simultaneously, the theory seems to me not tenable from whatever point of view it be examined. Anatomical, physiological, and pathological facts equally militate against it.

In the first place, it is not easy to understand how the abductors, being in number and aggregate amount of muscular tissue inferior

to their antagonists, could, anatomically considered, be the stronger ones, as supposed in Rosenbach's hypothesis.

Secondly, the abductors and adductors of the vocal cords, though, in a certain sense, obviously antagonistic to each other (in that they serve the diametrically opposed functions of opening and closing the glottis), yet are not in the same sense antagonists as the extensors and flexors of a limb. In the case of the latter, the different groups of muscles presiding over the movements of the part serve purposes *identical* in nature and in physiological value; in the case of the laryngeal muscles, however, not only are the functions different in physiological importance, but certainly also to some degree quite *independent* of one another. Whilst it is undoubtedly true that no phonatory effect can take place without respiratory movements at the same time coming into play—for phonation is a sort of modified expiration—yet the reverse of this does not hold good, respiration being in no way necessarily connected with phonation. There is, therefore, no reason why, in the performance of a function which is independent of that subserved by the antagonistic group of muscles, the latter ought to come constantly into play.

Rosenbach's comparison, therefore, of the laryngeal muscles with those of the limbs cannot in this respect be admitted to be unreservedly applicable.

Thirdly, if both groups of muscles, the abductors and adductors, were in reality constantly and simultaneously innervated during quiet respiration, and if the abductors merely predominated, one would naturally expect that, in cases of isolated paralysis of the glottis closers, such as in functional aphonia, the glottis should appear much wider in quiet respiration than when seen under ordinary circumstances. For, under such circumstances, the respiratory tonic innervation of the glottis openers would continue to the same degree, whilst the antagonistic innervation of the glottis closers, which, according to Rosenbach's theory, must previously have counterbalanced, to some extent, their abducting force, was absent. Yet the glottis of persons suffering from bilateral paralysis of the glottis closers during quiet respiration is not at all wider open than that of normal persons. This again speaks very forcibly against the hypothesis of a simultaneous innervation of the adductors and abductors with preponderance of the latter.

Fourthly, the fact independently demonstrated by Rosenbach himself in the paper referred to and by me* in several publications, viz.,

* (*a.*) "Clinical remarks on the proclivity of the abductor fibres of the recurrent laryngeal nerve to become affected sooner than the adductor fibres, or even exclusively, in cases of undoubted central or peripheral injury or disease of the roots or trunks of the pneumogastric, spinal accessory, or recurrent nerves" ('Archives of Laryngology,' vol. 2, 1881, p. 203).

that the abductors of the vocal cords are more easily disabled by any organic mischief acting upon their nerve supply than the adductors, and that they die sooner after the death of the individual than the adductors,* can hardly be reconciled with the idea of a preponderance of their physiological strength over that of the adductors.

Fifthly, the central conditions of the innervation of the two laryngeal groups of muscles also tell, as I hope to show in a paper which I shall shortly bring before the Royal Society in conjunction with Professor Victor Horsley, against the physiological preponderance of the abductors over the adductors.

Sixthly and lastly, the *coup de grâce* is given to this idea by the fact that stimulation of the cut end of the recurrent laryngeal in most species of animals (except the cat) results—if no undue influence of the anæsthetic used during the experiment comes into play†—in the corresponding vocal cord being drawn towards the middle line, *i.e.*, the adductors preponderate over the abductors, though both groups of fibres are equally strongly stimulated. This fact, needless to say, is wholly incompatible with the idea of preponderance of the abductor over the adductor muscles.

Thus from whatever point of view the question of the simultaneous innervation of the adductors and abductors, with preponderance of the latter during quiet respiration, be looked upon, there is no evidence for the existence of such a condition, and there are many arguments against it.

It is, indeed, much more probable that there is primarily a strict differentiation between the two antagonistic groups of laryngeal muscles (the phonatory and respiratory ones) and that, though there is under certain circumstances a *transition* of the functions of the one into those of the other, yet for the purposes of respiration under ordinary circumstances the respiratory muscles, *i.e.*, the posterior crico-arytenoid muscles, *alone* are engaged, being during inspiration and during expiration in a state of *semi-tonus*, in order to counterbalance the partial obstruction created by the interpolation of the phonatory into the respiratory apparatus.

This idea, indeed, more or less clearly expressed, has been before the minds of a good many of those who since the beginning of this century have worked in this field of investigation. It has been shown above that Legallois was quite conscious of the necessity of such a tonus existing. Luschka, again (*loc. cit.*), and Schech (*loc. cit.*)

(*b.*) "Ueber die Lähmung der einzelnen Fasergattungen des Nervus laryngeus inferior (recurrens)" ('Berl. Klin. Wochenschrift,' 1883, No. 46, *et seq.*).

* "On an apparently peripheral and differential Action of Ether upon the Laryngeal Muscles." By Felix Semon and Victor Horsley ('British Medical Journal,' 4 and 11 Sept., 1886).

† Compare the last-named paper by Semon and Horsley, p. 31.

speak very clearly of the necessity of the existence of a similar arrangement. The full physiological importance of it, however, appears to have only comparatively recently occurred to Krause and to myself, independently of one another, and to have been brought forward equally independently and simultaneously at the International Congress of 1884, as stated at the beginning of this paper.

The existence of such a tonus fully explains the difference between the conditions of the glottis as seen during quiet respiration and after death, and explains also why the interposition of the phonatory apparatus in the air passages has not been followed by any change in the type of normal respiration in man. In virtue of their preventing such a change, *i.e.*, of either increased labour on the part of the regular muscles of respiration or of the accessory muscles of respiration having to work constantly even during quiet respiration, the glottis openers, *i.e.*, the posterior crico-arytenoid muscles, appear to me *to deserve undoubtedly a much higher position in the mechanism of respiration than has been so far accorded to them.*

The only remaining question then would be: is this tonus of the abductor muscles an *automatic* one? *i.e.*, is it induced in the respiratory centre *itself*, or is it of the nature of a *reflex* tonus, *i.e.*, only engendered in the respiratory centre through *peripheral* influences?

Athough it has been shown by Rosenthal that the respiratory centre in the medulla oblongata, even after the section of both pneumogastric nerves, and after removal of the cerebrum as well as after section of the cervical part of the spinal cord, is capable of engendering rhythmic movements (so that its action in a certain sense must certainly be looked upon as an automatic one), yet at the same time there can be no doubt as to the existence of afferent impulses communicated to it along the most various peripheral nerves, and most of all along the main trunk of the pneumogastric. Whilst therefore, *a priori*, it would not at all be impossible that the tonus of the abductors of the vocal cords might *originate* in the respiratory centre itself, it seemed, in concord with general experiences concerning the nervous mechanism of respiration, at least equally probable, that impulses might be conducted *rhythmically* along the afferent fibres of the pneumogastric nerves to the respiratory centre, and there be changed into a *tonic* semi-innervation of the posterior crico-arytenoid muscles, which again under the influence of any of the extraneous causes above mentioned could be changed into rhythmical impulses coincident with and renewed with every respiratory movement.

It occurred to me that a more definite solution of this question might be hoped for, if it were possible to cut both pneumogastric nerves *below* the points from which the recurrent laryngeal nerves are given off. One could not hope that this experiment would defi-

nitely settle the question, because in animals, especially under the influence of anæsthetics, only rarely is a condition observed during respiration analogous to that seen in quiet respiration in man, their vocal cords, on the contrary, making very energetic rhythmical excursions. Still, it seemed legitimate to submit this question to experimental proof, because, even with these respiratory excursions, it was to be expected that if respiratory influences governing the action of the glottis-openers reached the respiratory centres along the trunks of the *pneumogastric* nerves, the excursions of the vocal cords after section of the latter would very much *diminish in intensity*, and, if they were *exclusively* conducted along these paths, that after section of the pneumogastrics below the points of departure of the recurrents, the glottis would not open any further than to the *cadaveric* position.

Professor Victor Horsley was kind enough to submit these theoretical considerations to experimental proof. On April 17, 1890, he, in the presence of Mr. Embleton and of myself, performed the following experiment: A small adult female fox terrier was etherised and tracheotomised; the narcosis was afterwards kept up with chloroform. First the right, afterwards the left, vagus was laid bare, and both nerves were cut about 1 centimetre below the points where the right recurrent laryngeal winds round the subclavian artery and the left round the aorta. As soon as the pleura was opened in order to get at the left vagus, artificial respiration was started and maintained until the end of the experiment.

Whilst previous to the cutting of the right vagus (and also after the division) the thorax as well as the vocal cords made *very extensive* and energetic rhythmical respiratory excursions (the glottis during inspiration being opened to its fullest extent), the respiratory excursions of the cords were, after section of the second vagus (the left), equally energetic but much less extensive, the glottis during respiration opening only to the cadaveric position.

The animal was killed by asphyxia; during its final forcible respiratory efforts the glottis again opened, during inspiration, to its *fullest* extent.

Dissection after death showed that both recurrent nerves were quite uninjured.

This experiment certainly went far to prove that respiratory impulses influencing the action of the posterior crico-arytenoid muscles reached the respiratory centre, and, more precisely speaking, the ganglionic centres of these abductor muscles, through the medium of the pneumogastric nerves. At the same time the full dilatation of the glottis during the asphytic stage of the animal, seemed to point out that the impulses thus engendered cannot be the only ones reaching these ganglionic centres, and that the respiratory centre, so far as the

larynx is concerned, may also be influenced through other afferent impulses.

To settle this point, if possible, more definitely, the experiment was repeated on May 8, 1890. A small castrated fox terrier was etherised and tracheotomised. Narcosis was afterwards kept up by ether. The right vagus was laid bare below the line of departure of the right recurrent laryngeal nerve. Both cords made very energetic and extensive rhythmical respiratory excursions. The right vagus was cut more than 1 cm. below the point of departure of the right recurrent laryngeal nerve. Both cords continued their excursions as if nothing had happened.

A subcutaneous injection of 5 grains of acetate of morphia was now made into the dog's thigh, the left vagus was then exposed, artificial respiration being started as soon as the pleura was opened previous to the section of the left vagus. It was again ascertained that the glottis still opened at *maximum* during inspiration. The left vagus was then cut about 1 cm. below the arch of the aorta, and it was now observed that, together with very considerable slowing of the two phases of respiration, the glottis, though it still opened widely, and certainly much beyond the cadaveric position, yet no longer opened *nearly* as widely as before, when the vocal cords during inspiration actually completely disappeared from view.

A very remarkable phenomenon was observed in connexion with the artificial respiration. Whenever the dog's lungs were *well* aerated, the excursions of the cords, though good, corresponded to the description given above of the conditions as occurring after section of the vagi, but as soon as asphyxia became threatening the glottis at once opened, as previously to the section, *ad maximum*.

The dog was killed by asphyxia. *Post-mortem* examination showed that both vagi had been cut very considerably below the points where the recurrents were given off, and that the latter were quite uninjured.

This experiment, then, in every respect tends to corroborate the conclusions drawn from the former, *i.e.*, that whilst the inspiratory impulses which act upon the ganglionic centres of the abductor muscles and which in animals more frequently induce rhythmical excursions of the vocal cords, but in man tonic semi-dilatation of the glottis, are of a *reflex* character, and are mainly conducted along the pneumogastric nerves, yet the latter are by no means the only source of this reflex innervation.

The question raised in the foregoing paragraphs will certainly demand still further elucidation, and the results arrived at in this paper will no doubt have to be checked by future observers. Still I think that, as the outcome of the investigation, so far as it has been conducted, the following conclusions may be drawn :—

First, the glottis in man is wider open during quiet respiration (inspiration and expiration) than after death or after division of the vagi or recurrent laryngeal nerves.

Second, this wider opening during life is the result of a permanent activity (tonus) of the abductors of the vocal cords (posterior crico-arytenoid muscles) which, therefore, belong not merely to the class of accessory, but of regular, respiratory muscles.

Thirdly, the activity of these muscles is due to tonic impulses which their ganglionic centres receive from the neighbouring respiratory centre in the medulla oblongata. It is very probable that these impulses rhythmically proceed to the respiratory centre from the stimulation of certain afferent fibres contained mainly, but not exclusively, in the trunks of the pneumogastric nerves, and that they are in the respiratory centre changed into tonic impulses. The regular activity of the abductors of the vocal cords during life, therefore, belongs to the class of reflex processes. The permanent half-contraction of these muscles, in which form their tonic innervation is manifested, can be further increased, in concord with the general laws of the mechanism of respiration, by either volition or other reflex influences.

Fourthly, in spite of their extra innervation, the abductors of the vocal cords are physiologically weaker than their antagonists.

Fifthly, these antagonists, the adductors of the vocal cords, have primarily nothing at all to do with respiration and ordinarily serve the function of phonation only. Their respiratory functions are limited to

(*a*.) Assistance in the protection of the lower air passages against the entry of foreign bodies;

(*b*.) Assistance in the modified and casual forms of expiration known as cough and laughing.

November 20, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Sir James Cockle, Mr. A. A. Common, Professor G. Carey Foster, the Rev. Professor Price, and Dr. Rae were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Determination of the Specific Resistance of Mercury in Absolute Measure." By J. V. JONES, Principal and Professor of Physics in the University College of South Wales and Monmouthshire, Cardiff. Communicated by Professor CLIFTON, F.R.S. Received August 8, 1890.

(Abstract.)

In the hope of paving the way for a more accurate determination of the ohm, the author has for a considerable time been engaged in submitting to the test of experiment certain modifications of the method of Lorenz which occurred to him as likely to lead to increased accuracy and certainty. The experiments have been made in the laboratory of the University College, Cardiff, with apparatus for the most part constructed in the College workshop. Five complete sets of observations were taken in the spring of this year, with the following results for the specific resistance of mercury at 0° C. :—

(i.)	94,103	absolute C.G.S. units.
(ii.)	94,074	,, ,, ,,
(iii.)	94,093	,, ,, ,,
(iv.)	94,045	,, ,, ,,
(v.)	94,021	,, ,, ,,
Mean..	94,067 $\pm$ 10 (probable error).	

The result may be otherwise expressed by saying that the ohm is

equal to the resistance of a column of mercury of 1 square mm. sectional area and 106·307 cm. long, the probable error being $\pm 0{\cdot}012$.

The author does not bring these numbers forward as the best determination possible by the method he has used. He is of opinion that if the apparatus be constructed on a scale a little larger and with a certain perfecting of detail, a single set of observations will give a result accurate to one part in 10,000, and that as a mean of a number of observations we may perhaps aim at the hundred-thousandth if regard is paid to the maintenance of definite temperatures in all parts of the apparatus, and if we can be said to know our length-standards to this degree of accuracy.

The observations were made by the method of Lorenz directly on mercury. The chief variations in the method introduced in the present investigation are as follows:—

(i) The elimination by a system of differential measurements of the errors that have so far attended the use of a mercury column as the measured resistance.

Lorenz himself took for his measured resistance the resistance of a mercury column contained in a glass tube, and the specific resistance was calculated from the dimensions of the column. It is hardly possible, however, that the latter calculation can have been, or is likely to be, achieved with accuracy, however accurately the tube be calibrated. For, on the one hand, if the wires from the disc (the terminal portions of which may be called the electrodes) are led to the ends of the tube, the equi-potential surfaces touched by them are not plane; and, on the other, if they are let into the tube at some distance from the ends, it is difficult to see how the distance between them is to be measured with the requisite accuracy.

These difficulties disappear if, instead of placing the mercury in a tube, it is placed in a long trough, and if, instead of measuring the distance between two electrodes, one electrode is kept fixed while measurement is made of the distance moved through by the other between two equilibrium positions corresponding to two different rates of rotation of the disc. The latter measurement it is easy to make with accuracy, for the movable electrode may be rigidly attached to the movable headstock of a Whitworth measuring machine or some other measuring bank placed parallel to the length of the trough; and the two equilibrium positions may be taken near the middle of the trough so as to avoid danger of curvature in the equipotential surfaces passing through the electrode in its two positions.

Let n_1, n_2 be the rates of rotation of the disc, and let l be the distance between the corresponding equilibrium positions of the movable electrode.

Then $$\mathrm{M}(n_1 - n_2) = \frac{l}{\mathrm{A}}\rho,$$

where M = the coefficient of mutual induction of coil and disc;
ρ = the specific resistance of mercury;
A = area of section of the mercury column.

The capillary depression at the sides of the trough would make it a serious task to determine the section of the mercury column by direct measurement to the required degree of accuracy. This difficulty is overcome by a further differential method, viz., by making observations with the mercury at two different heights in the trough.

Let b = the breadth of the trough;
$h_2 - h_1$ = the difference of height of the mercury surface in the two cases; and let
A = the section of the mercury column when the mercury is at the lower position.

Then we have, denoting by dashed letters the new values of the rates of rotation and the distance between the corresponding equilibrium positions—

$$\mathrm{M}(n_1 - n_2) = \frac{l}{\mathrm{A}}\rho,$$

and $$\mathrm{M}(n'_1 - n'_2) = \frac{l}{\mathrm{A} + b(h_2 - h_1)};$$

whence, eliminating A,

$$\rho = \frac{\mathrm{M}b(h_2 - h_1)}{\dfrac{l'}{n'_1 - n'_2} - \dfrac{l}{n_1 - n_2}}.$$

It is assumed in the above formula that the sides of the trough in that part of it traversed by the movable electrode are plane, parallel, and vertical.

The trough used in the experiments described was cut in paraffin wax, contained in a strong casting of iron, with its sides strengthened by outside ribs. The channel is approximately 43·5 inches long by 1·5 inches broad by 3 inches deep. It was first cut by a cutter rotating about 2000 times a minute, attached to the slide rest of the College Whitworth lathe, and subsequently finished by a scraper, attached in similar fashion, which took a very thin cut off sides and bottom. The result of the scraping was a very smooth and highly-finished surface.

(ii) The use of a standard coil with a single layer of wire, the coefficient of mutual induction of the coil and circumference of the disc being calculated by a formula obtained by the direct integration of the expression

$$\iint \frac{ds\, ds}{r} \cos \epsilon$$

for a circle and coaxial helix.

(iii) The use of a new form of contact brush at the disc circumference, which procures greatly increased steadiness in the galvanometer needle.

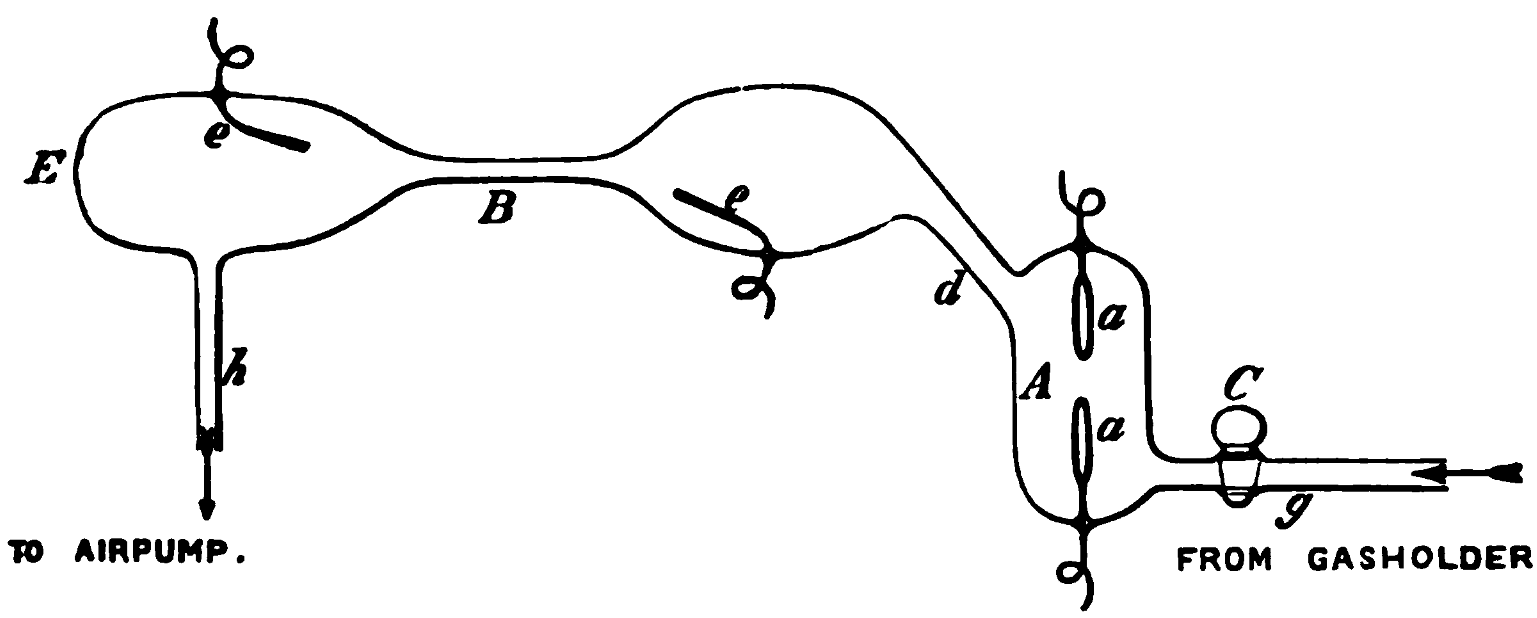
The brush consists of a single wire of phosphor bronze, perforated by a channel through which a continuous flow of mercury is maintained from a cistern of adjustable height.

Incidentally, a description is given of an accurate method of measuring the vibration frequency of a standard tuning-fork by means of a Bain's electrochemical telegraph receiver.

In conclusion, suggestions are made towards a new determination of the ohm that shall be final for the practical purposes of the electrical engineer.

II. "The Spectroscopic Properties of Dust." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received August 16, 1890.

The suggestion that the auroral spectrum, the principal ray in the spectrum of nebulæ, and other rays of unknown origin, might be due to meteoric dust induced us to investigate the problem whether solid particles of sufficient minuteness would act like gaseous molecules in an electric discharge and become luminous with their characteristic special radiation. The dust we employed was that thrown off from the surface of various electrodes by a disruptive discharge, and it was carried forward into the tube of observation by a more or less rapid current of air or other gas. The arrangement will be best understood from the annexed diagram, which represents a section of the glass vessel which was the principal part of the apparatus. *A* represents a bulb in which were the electrodes *a*, *a* to give the dust, connected by a widish tube *d* with the tube for observation *B*. The end *E* was blown clear, so that the narrow part of *B* could be observed end-on. The electrodes *e*, *e* were of platinum. The tube *g*, passing from *A* to the supply of gas, was fitted with a glass stopcock



C for regulating the intake, and the tube *h* led from the distant end of *B* to the air-pump. The air-pump was a large one worked by a gas-engine capable of keeping the pressure down to a few millimetres, even with a considerable leakage. Observations were made of the discharge in *B* at various low pressures, sometimes with, and at other times without, a Leyden jar in circuit. The sparks in *A* were generally taken with a jar, and there was ample proof, if proof were needed, of the dust derived from the electrodes, since it formed a visible deposit in the tube *d*, in the first bulb of *B*, and even on the end *E*. The air or other gas passed into *A* was filtered through cotton-wool to remove all dust before admission to the apparatus.

Various metals were used as electrodes in *A*, magnesium, iron, manganese, cadmium, fused calcium chloride, metallic sodium in a little glass cup on a platinum wire, and fragments of the Dhurmsala meteorite; but in no case could the rays of any of the substances employed be seen in the discharge through *B*, either when a Leyden jar was in circuit or not.

Incidentally, we found that magnesium electrodes were not so good as some of the other metals for these experiments, because the apparatus was never wholly free from traces of air, and lines or bright edges of bands of nitrogen fall very near the most characteristic lines of magnesium, and with small dispersion might easily be mistaken for them.

Air, hydrogen, carbon dioxide, and oxygen, were successively used as the gases passing through the apparatus, and at various pressures from 2 mm. up to 20, and, in some cases, up to 40 mm., but with the same result; no rays, due to the electrodes in *A*, could be detected in *B*. Even when one of the electrodes in *A* was sodium, and the sodium rays, orange, yellow, citron, green, and blue, were brilliant in the spectrum of *A*, not even the D lines could be detected in *B*. We should have expected that some traces of sodium *in the state of vapour* would have been carried by the stream of hydrogen into *B*; but it seems that it was not so; nor could the apparent absence of rays

due to the dust, be ascribed to mere faintness in their light, for we took photographs of the spectrum of B, and found that even lengthened exposures produced no evidence of rays due to the dust; nor could it be ascribed to the character of the discharge in B, for the discharge was varied; sometimes A and B were in the same circuit; sometimes the discharge in B was from a separate coil, and even the powerful discharge from a large coil stimulated by a De Meritens' magneto-electric machine, was tried.

That abundance of dust was formed by the sparking in A was proved not only by the deposit in the tube, but by allowing the stream of gas at atmospheric pressure from the tube h (of course disconnected from the pump) to impinge on a flame, when the characteristic flame-spectrum of the electrodes in A was at once manifest. When the gas used was hydrogen, and it was burnt in oxygen, the spectrum of the electrodes was particularly well seen; also when the gas was oxygen and led into a hydrogen flame.

That the dust was of extreme fineness and capable of being carried by a stream of gas to a great distance was proved as follows:—A stream of hydrogen, at ordinary pressure, was passed through the sparking tube with magnesium electrodes, and then through more than 100 feet of metal tube in a coil, and, finally, burnt as it issued. Before the sparking began there were no signs of magnesium in the flame; but when sparks had been passing between the magnesium electrodes for a short time, the magnesium spectrum was seen in the flame. It took 55 seconds for the gas to carry the dust through the long pipe, and when the sparking ceased it was again about the same time before the magnesium disappeared from the flame. It always appeared and disappeared sharply in correspondence with the sparking. Similar experiments, but with a shorter tube, were made with other metals, iron, sodium, lithium, &c., always with like results; also a current of oxygen was passed through the sparking tube and into a flame of hydrogen, and produced similar effects. Even aluminium, which does not usually show any part of its spectrum when used as an electrode in a vacuous tube, gave, when sparked in oxygen, dust which, when carried into a hydrogen flame, showed the characteristic bands of alumina.

Considering that a sensible amount of dust was deposited in the bulbs of B, we should have expected that some would be deposited on the electrodes e, e in that tube, and that the discharge from electrodes so coated would give the spectrum of the metal on their surface. There is no doubt that when no discharge was taking place in B the electrodes e, e did receive their share of dust; and, if it had been allowed to accumulate so as to form a coherent crust, it would have given its characteristic spectrum on first passing sparks in B. But, so long as the dust is loose, the passage of a discharge instantly clears

the electrodes of all dust, and seems to dispel all dust from the gas through which the discharge occurs. It is well known that an electric discharge in a vessel of air has the effect of clearing out of the air all the particles that serve as nuclei for the condensation of water; and we made several experiments with a view to determine whether a similar effect was produced on the dust in our tubes. The gas from the sparking tube was carried through a glass globe, and so on to the jet where it was burned; a wire connected with one pole of a Voss or Wimshurst electric machine projected into the interior of the globe, and a patch of tinfoil on the outside of the globe was connected with the other pole of the electric machine. So long as the Voss machine was not worked, the gas carried the dust from the sparking tube through the globe, and it was seen in the spectrum of the flame, or simply in the colour of the flame when lithium was one of the electrodes; but, on working the machine so as to produce a silent discharge inside the globe, the flame, in one or two seconds, suddenly ceased to show the spectrum of the dust, and in the case of the lithium lost its red colour. When the machine was no longer worked, the spectrum or colour speedily reappeared, to vanish again suddenly when the machine was started afresh. When a narrow tube, with a piece of tinfoil outside and a wire inside, was substituted for the globe, the like results ensued.

It appears, then, not only that dust, however fine, suspended in a gas will not act like gaseous matter in becoming luminous with its characteristic spectrum in an electric discharge, but that it is driven with extraordinary rapidity out of the course of the discharge. If, then, the spectrum of the aurora be due, not to the ordinary constituents of our atmosphere, but to adventitious matter from planetary space, we conclude that such matter must be in, or must be brought into, the gaseous state, or at least have its properties entirely altered from those it possesses at ordinary temperatures, before it becomes luminous in the electric discharge.

III. "On the Specific Heats of Gases at Constant Volume. Part I. Air, Carbon Dioxide, and Hydrogen." By J. JOLY, M.A., B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Professor FITZGERALD, M.A., F.R.S., F.T.C.D. Received September 2, 1890.

(Abstract.)

In this first notice the specific heats, at constant volumes, of air, carbon dioxide, and hydrogen are treated over pressures ranging from 7 to 25 atmospheres. The range of temperature is not sensibly

varied. It is found that the specific heats of these gases are not constant, but are variable with the density. In the case of air the departure from constancy is small and positive; that is, the specific heat increases with increase of the density. The experiments afford directly the mean value 0·1721 for the specific heat of air at the absolute density of 0·0205, corresponding to the pressure of 19·51 atmospheres. A formula based on the variation of the specific heat with density observed in the experiments ascribes the value 0·1715 for the specific heat at the pressure of one atmosphere. The formula assumes the specific heat to be a linear function of the density, which must as yet be regarded only as an approximation, the exact nature of the relation being concealed by variations among the experiments.

These results appear to be in harmony with the experiments of Wiedemann on the specific heat at constant pressure, and of Rowland on the mechanical equivalent of heat, from which the value 0·1712 is deduced for C_v at 760 mm.

The experiments on carbon dioxide reveal a more rapid variation of the specific heat with density, the variation in this case being again positive in sign. The formula

$$C_v = \rho \times 0{\cdot}2064 + 0{\cdot}16577$$

appears with considerable reliability to express the relation between specific heat and density.

The relation between specific heat and density in the case of hydrogen is of a negative character; that is, the specific heat diminishes with increase of density. The experiments are chiefly directed to elucidate this point, for, owing to the difficulty of preparing pure hydrogen, it was found that variations in the quantitative results of experiments on different samples of the gas were unavoidable. Accordingly the experiments were directed to a comparison of the specific heats of like samples of the gas at different densities. The variation with density is small, but (with one exception) all experiments on the purer hydrogen ascribe a negative character to it.

The nature of these variations of specific heat with change of density is, in the case of the three gases, in accord with their behaviour as regards Boyle's law, within the limits of pressure.

The experiments were effected in the steam calorimeter, a differential method being used in which an empty or idle vessel is thermally compared with the vessel holding the gas at high pressure. The vessels possessing approximately the same calorific capacity, the result, theoretically, is as if the gas was dealt with isolated from any containing vessel. Although practically this is not attained, many sources of error are eliminated by the procedure adopted.

IV. "Magnetism and Recalescence." By J. Hopkinson, D.Sc., F.R.S. Received October 9, 1890.

In my experiments the results of which are published, 'Phil. Trans.,' 1889, A, p. 443, I showed that recalescence and the disappearance of magnetisability in iron and steel occurred at about the same temperature. The evidence I then gave was sufficiently satisfactory, but did not amount to absolute proof of the identity of the temperatures. Osmond has shown that the temperature of recalescence depends upon the temperature to which the iron has been heated, also that it differs when the iron is heated and when it is cooled. He also showed that for some sorts of steel the heat is liberated at more than one temperature, notably that in steel with 0·29 per cent. of carbon heat is liberated when cooling at 720° C. and at 660° C., and that with steel with 0·32 per cent. carbon there is a considerable liberation of heat before the temperature is reached when this becomes a maximum. It appeared to be desirable to obtain absolute proof that the change of magnetic property occurred exactly when heat was liberated and absorbed, and to examine, magnetically, Osmond's two temperatures of heat liberation. I have not been able to obtain samples of steel of the size I used, showing two well marked temperatures of heat liberation and absorption, but I have a ring in which there is liberation of heat extending over a considerable range of temperature.

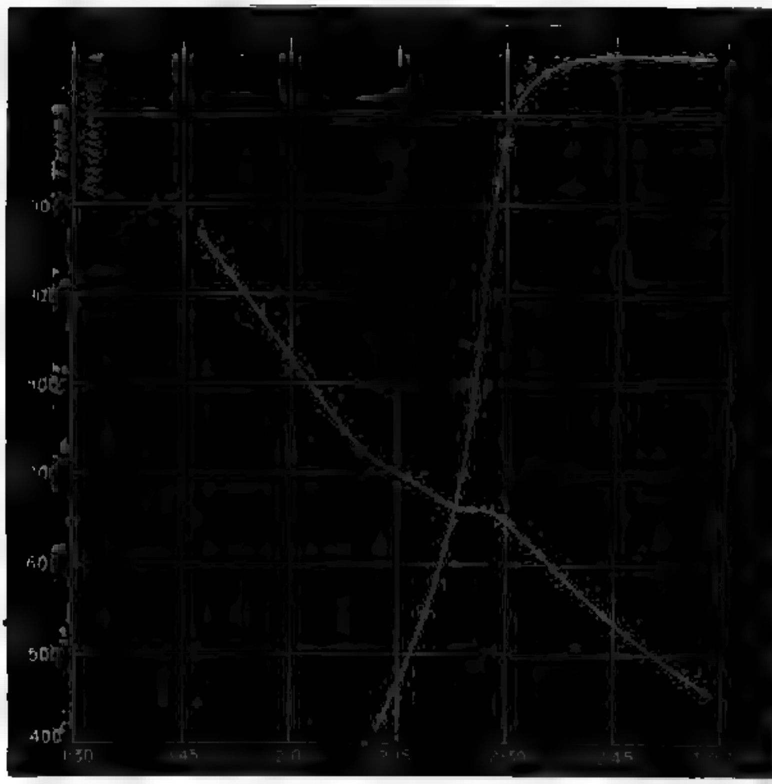
The samples had the form of rings of the size and shape indicated in fig. 1. A copper wire was well insulated with asbestos and laid in the groove running round the ring, and was covered with several layers of asbestos paper laid in the groove. This coil was used for measuring temperature by its resistance. The whole ring was served over with asbestos paper and with sheets of mica. The secondary exploring coil was then wound on, next a serving of asbestos paper and mica, and then the primary coil, and, lastly, a good serving of asbestos paper was laid over all. In this way good insulation of the secondary coil was secured, and a reasonable certainty that the temperature coil took the precise temperature of the ring, and that at any time the ring was throughout at one and the same temperature. The whole was placed in an iron pot, and this again in a Fletcher gas furnace. Observations were made of temperature as the furnace was heating, and from time to time of induction. In each case the time of observation was noted. Similar observations were made as the ring cooled, the furnace being simply extinguished. We are thus enabled to compare directly at the same instant the condition of the same ring as regards magnetism and as regards temperature, and,

Fig. 1.

therefore, qualitatively as regards its absorption or liberation of heat.

In fig. 2 are the results for a ring containing 0·3 per cent. of carbon or thereabouts. In this case only a cooling curve was taken. It will be observed that there is a considerable liberation of heat, beginning at 2 h. 12 m., temperature 715° C., and continuing to time 2 h. 22 m., temperature 660° C., being apparently somewhat slower at the end. This may, however, be only apparently slower, as the furnace temperature would fall lower in relation to the ring. At time 2 h. 22 m., temperature 660° C., the rate of liberation becomes much more rapid, so much so that the temperature for a time remains almost stationary. At time 2 h. 29 m. the liberation of heat appears to have ceased and the normal cooling to continue. Now, comparing the kicks of the galvanometer, which are proportional to the induction, we observe that the ring begins to be magnetisable at time 2 h. 12 m., its magnetic property increases till time 2 h. 22 m.; after this point the magnetisability increases much more rapidly, and is practically fully developed at 2 h. 31 m. In this case the development of magnetic property follows precisely the liberation of heat, observed both at the temperature of about 700° C. and at 660° C. We may, therefore, be certain, that both at the higher and lower temperatures of recalescence there is magnetic change, and that the one is as much dependent on the other as the solid condition of ice is upon the liberation of heat when water solidifies. The two changes

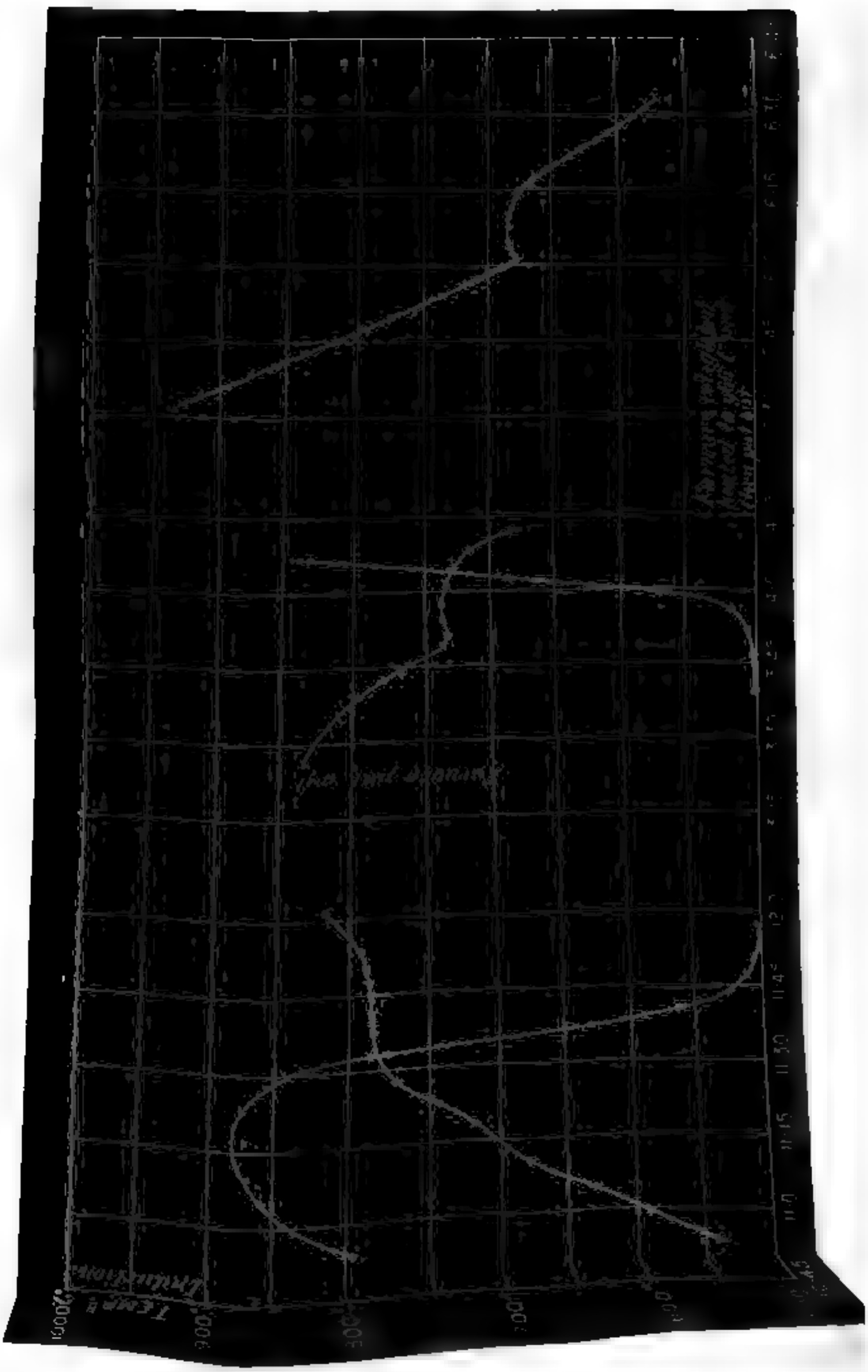
Fig. 2.



occur, not only at the same temperature, but simultaneously. A considerable magnetising force, 6·56, was taken, as it was expected and found that the magnetic property would then be more apparent when it was in the intermediate condition between the two temperatures of recalescence.

In fig. 3 are the results of a ring containing 0·9 per cent. of carbon. In this case we have a curve of heating and of cooling with magnetic property for comparison, and also a second cooling curve to show the recalescence temperature when the heating had been higher. Unfortunately I had forgotten to record the magnetising force; it was, however, much less than in the last case, probably less than unity. Looking at the curve, we see that there is a slight absorption of heat at time 11 h. 17 m., temperature 710° C. with doubtful effect on the magnetism. At time 11 h. 27 m., temperature 770° C., powerful absorption of heat begins and continues to time 11 h. 55 m., temperature 808° C.; it is between these times that the magnetisability is de-

Fig. 3.



creasing, and at the latter time that it finally disappears. The heating was continued to about 840° C., and the flame was then put out. In cooling, heat is liberated at one point only, and in this case with a distinct rise of temperature. The recalescence begins at time 3 h. 47 m., temperature 750° C., and it is precisely at this time that the ring begins to be magnetisable. The recalescence continues to time 4 h. 8 m., and at this time, and not before it, the magnetisability practically attains a maximum. Before the last portion of the curve the ring was heated to 966° C. Here no observations were made magnetically. This part of the curve, therefore, only shows the effect of higher heating in lowering the temperature of recalescence.

These experiments show that the liberation and absorption of heat, known as recalescence, and the change in magnetic condition, occur simultaneously. Also that in the case of steel with 0·3 per cent. of carbon both temperatures of liberation of heat are associated with change of magnetic condition.

Presents, November 20, 1890.

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Three bronze Medals, two bearing bust of Linnæus, and one commemorative of Joseph Hume, F.R.S.

Mr. J. Evans, Treas. R.S.

November 27, 1890.

Mr. JOHN EVANS, D.C.L., Treasurer and Vice-President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—Sir William Thomson, D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Archibald Geikie, LL.D.

Other Members of the Council.—Professor William Edward Ayrton; William Henry Mahoney Christie, M.A.; Professor W. Boyd Dawkins, M.A.; James Whitbread Lee Glaisher, D.Sc.; Hugo Müller, Ph.D.; Professor Alfred Newton, M.A.; Sir William Roberts, M.D.; William Chandler Roberts-Austen, F.C.S.; Professor Edward Albert Schäfer, M.R.C.S.; Sir George Gabriel Stokes, Bart., M.A.; Lieut.-General Richard Strachey, R.E.; Professor Joseph John Thomson, M.A.; Professor Thomas Edward Thorpe, B.Sc.; Sir William Turner, M.D.; Professor Sydney Howard Vines, M.A.; General James Thomas Walker, C.B.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Homology between Genital Ducts and Nephridia in the Oligochæta." By FRANK E. BEDDARD, M.A., Prosector of the Zoological Society. Communicated by Professor E. RAY LANKESTER, M.A., LL.D., F.R.S. Received October 10, 1890.

It is usually stated in text books that the genital ducts of the Oligochæta are homologous with nephridia; but nevertheless the question is one which has not yet been satisfactorily settled, for the

total independence of the two structures in *Lumbricus* and those aquatic Oligochæta of which the development is known is a difficulty in the way of accepting this view. Claparède, who first clearly formulated the arguments in favour of regarding the genital ducts as slightly modified nephridia, made a mistake in stating that the genital segments of the aquatic Oligochæta contain no nephridia; this error was pointed out by Vejdovský* who discovered that the genital segments are originally furnished with nephridia, which atrophy on the ripening of the sexual products and the appearance of their ducts. Professor Lankester pointed out that in *Lumbricus*† the genital ducts and the nephridia have a close relation to one or other of the two pairs of setæ with which each segment is provided. He suggested that the genital ducts might represent the only portion left of a ventrally opening series of nephridia. M. Perrier's memorable investigations‡ into the structure of exotic Earthworms tended at first to confirm this theory. He discovered that in one Earthworm (*Plutellus*) the nephridia alternated in position from segment to segment, thus suggesting that the supposed original two sets of nephridia had both partly persisted and partly disappeared. In other forms the nephridia were found to be related to the ventral setæ, and the genital apertures to the dorsal setæ, the exact converse of the condition which occurs in *Lumbricus*. Later investigations, however, which resulted in the discovery that the genital apertures and nephridiopores may coincide at the same seta, led M. Perrier to abandon the hypothesis. My own discovery, first published in the Proceedings of this Society,§ that in *Acanthodrilus multiporus* there are more than a single pair of nephridiopores to each segment, removed the difficulties urged by Perrier. And as this discovery has been extended by myself and by others to many species and genera of Earthworms, there can be no longer any intrinsic improbability in the hypothesis. The whole subject has been lately reviewed by Eisig in his treatise upon the Anatomy and Physiology of the Capitellidæ, which forms one of the series of monographs issued by the Zoological Station at Naples. Dr. Eisig decides that the genital ducts are probably modified nephridia in the Oligochæta; in the Capitellidæ they certainly are; but, as the Capitellidæ do not appear to me to be so nearly related to the Oligochæta as Dr. Eisig considers, I should regard this argument as only having the force that an argument from analogy can have. Since the appearance of Dr. Eisig's work, an important paper by Dr. Stolc|| dealing with the generative organs

* 'System u. Morph. d. Oligochäten,' Prag, 1884.
† 'Quart. Journ. Microsc. Sci.,' 1864–5.
‡ 'Nouv. Arch. du Muséum,' vol. 8 (1872).
§ 'Roy. Soc. Proc.,' vol. 38, 1885, p. 459.
|| 'Böhm. Gesell. Sitzber.,' 1889.

of *Æolosoma* has come into my hands; it appears that in this Annelid there are no special sperm ducts, but that the function of such ducts is performed by several pairs of nephridia. This fact, however, interesting though it is, is not a proof of the homology between sperm ducts and nephridia in other types.

I have lately had the opportunity of studying the development of the New Zealand species *Acanthodrilus multiporus*. The sum of money which the Government Grant Committee of the Royal Society were good enough to place at my disposal has enabled me to defray the expenses of this investigation.

In the young embryos of this worm each segment is furnished with a pair of nephridia, each opening by a ciliated funnel into the segment in front of that which carries the dorsally placed external pore. In later stages the funnels degenerate, and that portion of the tube which immediately follows the funnel becomes solid, losing its lumen; at the same time the nephridium branches, and communicates with the exterior by numerous pores. At a comparatively early stage, four pairs of gonads are developed in segments X—XIII; each of these is situated on the posterior wall of its segment, as in *Acanthodrilus annectens*, and not on the anterior wall, as in the majority of Earthworms. When the gonads first appear, the nephridial funnels, with which they are in close contact, are still ciliated, and their lumen is prolonged into the nephridium for a short distance. Later the cilia are lost, and the funnels increase greatly in size, while those of neighbouring segments—in fact, all the remaining funnels—remain stationary for a time, and then become more and more degenerate. The large funnels of the genital segments become the funnels of the vasa deferentia and oviducts; it will be observed that the number of ovaries and oviducal funnels (*two* pairs) at first corresponds to that of the testes and sperm duct funnels; subsequently the gonads and commencing oviducts of segment XII atrophy. Each of these large funnels is continued into a solid rod which passes back through the septum, and then becomes continuous with a coiled tuft of tubules, in which there is an evident lumen, and which is a part of the nephridium of its segment. In the segments in front of and behind the genital segments, the rudimentary funnels communicate in the same way with a solid rod of cells which runs straight for a short distance and then becomes coiled and twisted upon itself and provided with a distinct lumen. In fact, apart from the relative size of the funnels and the presence of the gonads, it would be impossible to state from which segment a given section through the terminal portion of a nephridium had been taken. In a later stage the large funnels of the genital segments become ciliated; but this ciliation takes place before there is any marked change in the tube which is connected with the funnel.

In the young worm which has just escaped from the cocoon, the funnels are ciliated, and they are each of them connected by a short tube, in which a lumen has been developed, but which ends blindly in close proximity to a coil of nephridia. No trace of any nephridial tube other than the sperm duct or oviduct could be observed, whereas, in the preceding and succeeding segments the rudimentary nephridial funnel and a straight tube leading from it direct to the body wall were perfectly plain. Dr. Bergh* has figured, in his account of the development of the generative organs of *Lumbricus*, a nephridial funnel in close contact with the funnel of the genital duct. It may be suggested that a corresponding funnel has been overlooked in the embryo *Acanthodrilus;* the continuity of a structure, identical (at first) with the nephridia of the segments in front and behind, with the genital funnels, seems to show that a search for an additional nephridial funnel would be fruitless.

I can only explain these facts by the supposition that *in Acanthodrilus multiporus the genital funnels and a portion at least of the ducts are formed out of nephridia.* This mode of development is a confirmation, to me unexpected, of Balfour's suggestion† that in the Oligochæta the nephridium is broken up into a genital and an excretory portion.

In the comparison of the facts, briefly described here, with the apparently independent origin of the generative ducts in other Oligochæta, it must be borne in mind that in *Acanthodrilus* the segregation of the nephridium into several almost detached tracts communicating with the exterior by their own ducts precedes the formation of the genital ducts.

II. "The Patterns in Thumb and Finger Marks: on their Arrangement into naturally distinct Classes, the Permanence of the Papillary Ridges that make them, and the Resemblance of their Classes to ordinary Genera." By FRANCIS GALTON, F.R.S. Received November 3, 1890.

(Abstract.)

The memoir describes the results of a recent inquiry into the patterns formed by the papillary ridges upon the bulbs of the thumbs and fingers of different persons. The points especially dwelt upon in it are the natural classification of the patterns, their permanence throughout life, and the apt confirmation they afford of the opinion that the genera of plants and animals may be isolated from one another otherwise than through the influence of natural selection.

* 'Zeitschr. Wiss. Zool.,' 1886.

† 'Compar. Embryol.,' vol 2, p. 617.

The origin of the patterns was shown to be due to the existence of the nail, which interfered with the horizontal course of the papillary ridges, and caused those near the tip to run in arches, leaving an interspace between them and the horizontal ridges below. This interspace was filled with various scrolls which formed the patterns. The points or point at which the ridges diverged to enclose the interspace were cardinal points in the classification. It was shown that there were in all only nine possible ways in which the main features of the inclosure of the interspace could be effected. In addition to the 9 classes there was a primary form, occurring in about 3 per cent. of all the cases, in which the interspace was not clearly marked, and from this primary form all the other patterns were evolved. The forms of the patterns were easily traced in individual cases by following the two pair of divergent ridges, or the one pair if there was only one pair, to their terminations, pursuing the innermost branch whenever the ridge bifurcated, and continuing on an adjacent ridge whenever the one that was being followed happened to come to an end. 25 of the principal patterns were submitted, and a few varieties of some of them, making a total of 40. They are by no means equally frequent.

The data as to the permanence of the patterns and of the ridges that compose them were supplied to the author by Sir W. J. Herschel, who, when in the Indian Civil Service, introduced in his district the practice of impressing finger marks as a check against personation. Impressions made by one or two fingers of 4 adults about 30 years ago, and of a boy 9 years ago, are compared with their present impressions. There are eight pairs of impressions altogether, and it is shown that out of a total of 296 definite points of comparison which they afford, namely the places where ridges cease, not one failed to exist in both impressions of the same set. In making this comparison, no regard was paid to the manner in which the several ridges appear to come to an end, whether abruptly or by junction with another ridge. The reason was partly, because the neck where junction takes place is often low and may fail to leave a mark in one of the impressions.

Lastly, the various patterns were shown to be central typical forms from which individual varieties departed to various degrees with a diminishing frequency in each more distant degree, whose rate was in fair accordance with the theoretical law of frequency of error. Consequently, wide departures were extremely rare, and the several patterns corresponded to the centres of isolated groups, whose isolation was not absolutely complete, nor was it due to any rounding off by defined boundaries, but to the great rarity of transitional cases. This condition was brought about by internal causes only, without the least help from natural selection, whether sexual or other. The

distribution of individual varieties of the same patterns about their respective typical centres was precisely analogous in its form, say, to that of the Shrimps about theirs, as described in a recent memoir by Mr. Weldon ('Roy. Soc. Proc.,' No. 291, p. 445). It was argued from this, that natural selection has no monopoly of influence either in creating genera or in maintaining their purity.

III. "Preliminary Note on the Transplantation and Growth of Mammalian Ova within a Uterine Foster-Mother." By WALTER HEAPE, M.A., Balfour Student at the University of Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. Received November 12, 1890.

In this preliminary note I wish merely to record an experiment by which it is shown that it is possible to make use of the uterus of one variety of rabbit as a medium for the growth and complete fœtal development of fertilised ova of another variety of rabbit.

Briefly, the experiment made was as follows:—On the 27th April, 1890, two ova were obtained from an Angora doe rabbit which had been fertilised by an Angora buck thirty-two hours previously; the ova were undergoing segmentation, being divided into four segments.

These ova were immediately transferred into the upper end of the fallopian tube of a Belgian hare doe rabbit which had been fertilised three hours before by a buck of the same breed as herself.

It may be well to mention here, I bought this Belgian hare doe some three months before; the man from whom I bought her bred her, and guaranteed her to be a virgin doe of about seven months old. During the time I had her, until the 27th April, she had never been covered by a buck of any breed, being kept *always* isolated from the various bucks in my rabbitry.

In due course this Belgian hare doe gave birth to six young—four of these resembled herself and her mate, while two of them were undoubted Angoras. The Angora young were characterised by the possession of the long silky hair peculiar to the breed, and were true albinoes, like their Angora parents.

As a proof of their parentage, I would add they inherit a habit which nearly all the Angoras I have kept affect—it was marked in their Angora mother and especially pronounced in their father—a habit of slowly swaying their head from side to side as they look at you. I mention this fact because I have never observed the same habit in any breed of rabbits except Angora.

It should be remembered also as a further proof that I put into the Belgian hare doe *two* fertilised ova from the Angora doe, and that *two* Angora young were borne by the former.

Three of the Belgian hare young unfortunately died during the months of September and October, from some undetermined cause (alimentary ?); one of the Belgian hare young—a doe—and the two Angoras—both bucks—survive, and appear fairly strong and hearty.

At the date on which I am writing, 7th November, 1890, the surviving young ones are twenty-three weeks old, having been born on the 29th May, 1890.

All the young at the time of their birth suffered more or less from some skin disease, which, however, disappeared under treatment, and one of the Angora young, who suffered the most from the skin disease, has been remarkably scantily supplied with hair, but this defect is becoming less and less obvious.

Both the Angora young when born were bigger and stronger than any of the other young, and they have all along retained their supremacy in this direction.

I can see no sign in the Angora young of any Belgian hare strain, and the Belgian hare young have not shown any likeness to their foster-brothers. The surviving Belgian hare inherits a white left fore-foot from her father, and one of those which died was similarly marked.

The peculiarities of the Angora young have been already noted.

The experiment described above was undertaken to determine in the first place what effect, if any, a uterine foster-mother would have upon her foster-children, and whether or not the presence and development of foreign ova in the uterus of a mother would affect the offspring of that mother born at the same time.

So far as this single case goes, the evidence is negative.

Before long, I propose to continue my experiments and to extend them.

In concluding this note, I would record my great indebtedness to Mr. Samuel Buckley, M.D. (Lond.), F.R.C.S. (Eng.), of Manchester, who has most kindly given me his valuable assistance in the necessary operative portion of the experiment.

IV. "The Conditions of Chemical Change between Nitric Acid and certain Metals." By V. H. VELEY, M.A., the University Museum, Oxford. Communicated by Professor ODLING, F.R.S. Received October 23, 1890.

(Abstract.)

This paper is in continuation of a preliminary communication on the same subject; the main points contained in it are as follows:—

I. The metals copper, mercury, and bismuth do not dissolve in

nitric acid of about 30 per cent. concentration (the acid commonly employed for the preparation of nitric oxide gas) and heated to a temperature of 30° C., provided that nitrous acid is neither present initially nor formed subsequently. To prevent this, it is necessary in the cases of copper and bismuth to add a small quantity of some oxidising substance, such as hydrogen peroxide or potassium chlorate, or, as less efficacious, potassium permanganate, or to pass a current of air or, lastly, such a substance as urea, which destroys the nitrous acid by its interaction.

II. If the conditions are such that these metals dissolve, then the amount of metal dissolved and the amount of nitrous acid present are concomitant variables, provided that the nitric acid is in considerable excess. Change of conditions, such as concentration of acid and variation of temperature, which increase the former increase also the latter.

III. If the conditions are such that these metals dissolve, it would appear that the metallic nitrite is at first formed, together with nitric oxide; the former is decomposed by the excess of nitric acid to liberate nitrous acid, while the latter reduces the nitric acid to form a further quantity of nitrous acid.

Eventually the net result is the product of two reverse chemical changes represented by the equations—

$$\text{(i.)}\ 2NO + HNO_3 + H_2O = 3HNO_2,$$
$$\text{(ii.)}\ 3HNO_2 = 2NO + HNO_3 + H_2O.$$

The nitrous acid is thus destroyed as fast as it is generated.

IV. If the conditions are such that metals dissolve in nitric acid, then nitrous acid is invariably the initial product of reduction.

V. The metals copper, mercury, and bismuth dissolve very readily in a 1 per cent. solution of nitrous acid; under these conditions nitric acid present in slight excess interferes with, rather than promotes, the chemical change. This result is probably due to the greater stability of nitrous acid in the presence of nitric acid.

VI. Hydrogen gas reduces nitric to nitrous acid in presence of cupric or lead nitrate; it also converts mercuric into mercurous nitrate, but does not produce any change in solutions of bismuth and zinc nitrates dissolved in nitric acid.

V. "The Variations of Electromotive Force of Cells consisting of certain Metals, Platinum, and Nitric Acid." By G. J. BURCH, B.A., and V. H. VELEY, M.A., the University Museum, Oxford. Communicated by Professor ODLING, F.R.S. Received October 23, 1890.

(Abstract.)

The description of the apparatus, the capillary electrometer, and the method of working are given fully in the paper. The following conclusions are drawn from the results of the experiments:—

I. When the metals copper, silver, bismuth, and mercury are introduced into purified nitric acid of different degrees of concentration, and a couple made with platinum, the electromotive force of such a cell increases considerably until it reaches a constant and (in most cases) a maximum value. The rise of E.M.F. is attributed to the production of nitrous acid by the decomposition of the nitric acid, and the final value is considered to be due to the former acid only, while the initial value is due for the most part to the latter acid, though it is affected to a remarkable degree by the amount of impurity of nitrous acid, either initially present or produced by minute and unavoidable uncleanliness of the metallic strip and the containing vessel.

II. If nitrous acid has been previously added to the nitric acid, then the maximum E.M.F. is reached *at once*.

III. If the conditions, namely, increase of temperature, of impurity, and of concentration of acid, are such as would favour a more rapid solution of the metal, and consequently a more rapid production of nitrous acid, then the rise of E.M.F. is concomitantly more rapid.

IV. Conversely, if the conditions are unfavourable to the production of nitrous acid, the rise of E.M.F. is less rapid.

V. If any substance, such as urea, be added which would tend to destroy the nitrous acid as fast as it may be formed, then the rise of E M.F. is extremely slow, being dependent upon the number of molecular impac s of the nitrous acid upon the surface of the metal.

Thus the results obtained by the electrometer and by the chemical balance are in every way confirmatory the one of the other.

The authors propose to conduct further investigations on cells containing other acids, to determine whether the action of them upon metals is conditioned by the presence of their products of electrolysis.

Presents, November 27, 1890.

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December 1, 1890.

ANNIVERSARY MEETING.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts, on the part of the Society, was presented, by which it appears that the total receipts, including balances carried from the preceding year, amounted to £7,665 9*s.* 10*d.* on the General Account, and £7,016 11*s.* 7*d.* on account of Trust Funds, and that the total expenditure in the same period amounted to £5,886 1*s.* 11*d.* on the General Account, and £4,457 7*s.* 8*d.* on account of Trust Funds, leaving a balance on the General Account of £1,753 9*s.* 5*d.* at the bankers', and £25 18*s.* 6*d.* in the hands of the Treasurer, and, on account of Trust Funds, a balance at the bankers' of £2,559 3*s.* 11*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary (Nov. 30, 1889).

On the Home List.

Beckles, Samuel Husbands, F.G.S.
Burt, T. Seymour, Maj., M.R.A.S.
Davis, Sir John Francis, Bart.
Duncan, James Matthews, M.D.
Ellis, Alexander John, B.A.
Fischer, William Lewis Ferdinand, M.A.
Gull, Sir William Withey, Bart., M.D.
Hood, Charles, F.R.A.S.
Jones, Charles Handfield, M.B.
Kane, Sir Robert, M.R.I.A.
Lefroy, Sir John Henry, General R.A.
Mylne, Robert William, F.R.S.E.
Napier of Magdala, Robert, Lord, G.C.B.
Parker, William Kitchen, F.Z.S.
Perry, Rev. Stephen Joseph, F.R.A.S.
Smyth, Sir Warington Wilkinson, F.G.S.
Talbot, Christopher Rice Mansel.

On the Foreign List.

Rosenberger, Otto August.

Defaulter.

Twiss, Sir Travers, Knt., Q.C.

Change of Name and Title.

Macdonald, Right Hon. John Hay Athole, *to* Lord Kingsburgh.

Fellows elected since the last Anniversary.

Baker, Sir Benjamin, M.Inst.C.E.
Bosanquet, Robert Holford Macdowall, M.A.
Burbury, Samuel Hawkesley, M.A.
Gardiner, Walter, M.A.
Kerr, John, LL.D.
Lea, Arthur Sheridan, D.Sc.
MacMahon, Percy Alexander, Major R.A.
Norman, Rev. Alfred Merle, M.A.
Perkin, Professor William Henry, F.C.S.
Pickering, Professor Spencer Umfreville, M.A.
Roberts, Isaac, F.R.A.S.
Sharp, David, M.B.
Teall, J. J. Harris, M.A.
Thorne, Richard Thorne, M.B.
Weldon, Walter Frank Raphael, M.A.

On the Foreign List.

Cannizzaro, Stanislao.
Chauveau, Jean Baptiste Auguste.
Rowland, Henry A.

The President then addressed the Society as follows:—

The proceedings of every Anniversary Meeting remind us of the losses which the Society has sustained during the preceding year by the deaths of several of its Fellows. Of those whom we have lost during the past year, some were distinguished for their scientific labours, some were well known in the world at large. Of several, full obituary notices have appeared or will appear in the 'Proceedings,' but it will, I think, be in accordance with the wishes of the Fellows that I should say a few words on the present occasion about some of those whom we have lost.

The Rev. Stephen J. Perry, S.J., whose name is well known in connexion with his accurate magnetic observations, and his labours in the domain of solar physics, was last year elected a member of the Council. It was known that he could not attend the first meeting, as he would be on his way to the West Indies to observe the total solar eclipse of the 22nd of December, but we looked forward

to having him at the meetings of our Council after that was over. In this we were doomed to disappointment, for a telegram which arrived shortly after the day of the eclipse brought the sad intelligence of his death. His illness began shortly before the day of the eclipse, but he did not suffer it to interrupt his preparations, but worked on to the end as far as his failing strength would allow. The observation of total solar eclipses was a branch of solar physics to which he had paid special attention, and, considering the circumstances of his death, we may regard him as a martyr to science, while at the same time his kindly disposition ensured attachment from all who knew him.

William Lewis Ferdinand Fischer, who was a friend of mine for more than forty years, was a German by birth, and was educated in Germany. He came to Cambridge later in life than most men commence residence, entering at my own college, Pembroke. At the time when he entered he was only imperfectly acquainted with the English language. He took his degree in due course, having come out fourth wrangler. He obtained a fellowship at Clare College, and, after some time, was elected Professor of Natural Philosophy in the University of St. Andrews, at which town he continued to reside till his death, and where his widow still resides. He was remarkably well acquainted with what had been done in physical science.

In the middle of January we lost, in Lord Napier of Magdala, a man of world-wide reputation, who had been a Fellow of the Society for more than twenty-three years. A sketch of his life has already appeared in the 'Proceedings.'

The eminent physician Sir William Gull, who for some considerable time had been in failing health, was taken from us at the beginning of this year. The Fellows have already in their hands a sketch of his life.

In February there died, at an advanced age, our Fellow Sir Robert Kane, who was eminent among what we may regard as the last generation of chemists. A biographical notice of him is already in the hands of the Fellows.

Robert William Mylne, who died in July, was for thirty years a Fellow of the Society, to which also both his father and grandfather belonged. He was twelfth in direct descent of a family of architects and engineers, his direct ancestor having had the erection of new buildings for Holyrood Palace in the reign of Charles the Second. He was himself a hydraulic engineer, and was frequently consulted as to water supply, both by the Government and by private companies. He was perhaps best known in the department of geology, and his geological map of London and the neighbourhood, a work of immense labour and expense, was long a standard authority among scientific men.

General Sir John Henry Lefroy, who died last April, combined the duties of his profession and of his responsible offices as Governor of Bermuda, and, for a time, of Tasmania, with active scientific work in relation especially to terrestrial magnetism, with which he was connected by his post as director of magnetic observatories at St. Helena and at Toronto. He is the author of a treatise on the subject, and has entered into some investigations bearing on a possible cause of local magnetic irregularities, which seem well deserving of consideration.

Sir Warington Wilkinson Smyth, who was so high an authority on all that relates to mining, and geology as bearing upon it, was one of our Fellows who repeatedly served on our Council and on various committees, and by his sound judgment aided us in our deliberations. Though his health had been failing for some time, his end came upon us with startling suddenness. It will be remembered by many of the Fellows that he was present at our conversazione on the 18th of June, and next morning he breathed his last. He was widely known as a man of science, and was honorary Fellow of various societies on the Continent. In him I have lost one who was formerly a colleague of my own as lecturer in the School of Mines.

William Kitchen Parker held a very high place among biologists in relation especially to the homologies of the vertebrate skeleton. Notwithstanding the laborious nature of his profession, he managed to find time for his scientific pursuits, and our 'Transactions' contain a large number of papers which came from his pen, and are illustrated by elaborate drawings. So highly were his biological researches thought of, that for several years means were found, through an application of a portion of the Government Grant, for enabling him to dispense with the laborious work of his profession, and devote himself to science. His genial disposition and vivacity of manner, and, curiously enough, his personal appearance, reminded one of Faraday.

Dr. James Matthews Duncan, who died suddenly from heart disease, was eminent as an obstetric physician, and was a man of singular straightforwardness of character.

Our Fellow Charles Handfield Jones died on the 30th of September. His chief scientific labours lay in the domain of pathological anatomy.

Alexander John Ellis, who died on October 28, devoted great attention to philology and to the theory of the perfection of musical sounds, and translated v. Helmholtz's work, "Tonempfindungen." He had, shortly before his death, received an honorary doctor's degree from the University of Cambridge.

During the last year we have lost two out of our three senior Fellows, Christopher Rice Mansel Talbot, a Fellow also of the Lin-

nean Society, who was elected our Fellow in 1831, and, still more recently, in fact only a few days ago, Sir John Francis Davis, who was elected as long ago as 1822, and died at the very advanced age of ninety-six.

The number of Fellows elected before 1847 is now reduced to twenty-three, so that in any statistical calculations of the effect of the statutes of 1847 on the number of Fellows, the present condition of the Society may be taken as practically normal.

The Committee appointed in 1888 to consider the best mode of administrating the fund, which was inaugurated in 1882, for founding a memorial to our late eminent Fellow, Charles Darwin, have now presented their report, which has been adopted by the Council. It has been decided that the proceeds of the Darwin Fund be for the present applied biennially in reward of work of acknowledged distinction (especially in biology) in the field in which Mr. Darwin himself laboured; that the award consist of a medal in silver or bronze, accompanied by a grant of £100; that it be made either to a British subject or to a foreigner, and without distinction of sex; and that the award should be conferred at the same time as other medals at the Anniversary.

It was further intended, in accordance with Mr. Darwin's known views, that, as a rule, the award should be made rather for the work of younger men in the early part of their career than as a reward to men whose scientific career is nearly finished.

The Committee appointed at a meeting of the Council held immediately before the last anniversary meeting of the Society, to set on foot a memorial of our late Fellow James Prescott Joule, have naturally not got quite so far in their work. They decided that the memorial should take an international character, and should have for its object the encouragement of research in physical science, and should also have in view the erection of some personal memorial in London. The subject was accordingly brought to the notice of a number of scientific men abroad, from many of whom favourable replies have been received. The Joule Committee have resolved, "That the balance of the fund, after providing a suitable personal memorial, be transferred to the President, Council, and Fellows of the Royal Society, and that the President and Council be requested to undertake the administration of the proceeds in such manner as may appear to them most suitable for the encouragement of research, both in England and abroad, especially among younger men, in those branches of physical science more immediately connected with Joule's work;" and, also, "That the treasurer be instructed to retain for the present a sum not exceeding £300 for the expenses of the medallion, and hand over the balance to the President, Council, and Fellows of the Royal Society."

This offer of the Joule Committee was accepted with thanks by the Council, but the further consideration of the steps to be taken has not yet been entered on. Meanwhile the treasurer of the fund has handed over to the treasurer of the Royal Society a sum of about £1,400.

In 1663, when the second charter was granted to the Society, a body of statutes was drawn up for regulating various matters not fixed by charter. Alterations have since been made from time to time, as provided for in the statutes themselves. The last considerable alteration was made in 1847, when the present system was introduced, according to which the Council select from the candidates, other than those who have a special privilege as to coming on for ballot, a definite number whom they recommend to the Society for election, and the election takes place on one definite day in the year. A few changes, of less importance, have been made since that time, and experience has pointed out the desirability of some changes of detail, chiefly as regards the mode of dealing with papers. A committee was appointed last session, and continued at the commencement of the present, to revise the whole body of statutes, with a view to bring them into stricter conformity with existing practice, and at the same time to propose further changes, should any such appear desirable. The Committee have now reported; but as the session was near its end, and the subject was one requiring full consideration by the Council, the report has been merely received and entered on the minutes, and it has been left to the Council that is to be elected to-day to take such further steps as may appear to them desirable.

Some of the proposed alterations relate to the mode of dealing with papers which are communicated to the Society, which is a matter of practical business that may well be left to the judgment and experience of the executive body. But some points have been raised which it seems desirable to bring to the notice of the Fellows at large, in order that they may have an opportunity of considering them before a final decision is come to by the new Council.

The question has more than once been raised whether, considering the increase of population and the more general diffusion of scientific knowledge which has taken place within the last forty years, the number of candidates to be selected by the Council for recommendation to the Fellows for election might not now, with advantage, be made a little larger than fifteen, the number at which it was fixed in 1847. On this question there was considerable difference of opinion in the Committee, but the majority were in favour of keeping the number as it is at present.

Connected, to a slight extent, with this question is another, whether the Council should not have the power of recommending to the Fellows for election, in addition to the fifteen selected from among

the candidates on the ground of scientific merit, a strictly limited number of men of very high eminence in other ways. The Committee recommend that such a power be entrusted to the Council, the number of Fellows who have been thus elected, existing at any time, being limited to a maximum of twenty-five, and the number elected in any year to a maximum of two.

The question was also discussed whether the maximum number of foreign members, which at present stands at fifty, should be increased, and was decided in the negative.

Another recommendation of the Committee which, perhaps, it may be as well to mention, is one enabling the Council, in any year, to regulate for the ensuing year the length of the Christmas and Easter holidays. At present the weekly meetings are resumed in the second week after Christmas week, and there is then no intermission till Passion week, although the earlier portion at least of this interval is a time during which papers intended for reading do not usually come in so frequently as towards the end of the session. According to the statute in force till 1888, three of the ordinary weekly meetings between Whit-Sunday and the last meeting in June were cut out, by the Whitsun holiday, Ascension Day, and the annual election of Fellows; and as at that time of year papers commonly come in pretty frequently, there was a considerable congestion of papers towards the close of the session. This congestion was partially relieved by an alteration of the statutes, which came into force in 1888, enacting that an ordinary meeting should be held at the conclusion of the Annual Meeting for the election of Fellows; but the fact that the proportion between the number of meetings held and the number of papers that come in varies a good deal with the season seems to render it desirable that the regulation of the number of meetings should be rather more elastic, and should, to some extent, be left in the hands of the Council.

Since the last anniversary twenty-five memoirs have been published in the 'Philosophical Transactions,' containing a total of 1068 pages and 72 plates. Of the 'Proceedings,' eleven numbers have been issued, containing 1165 pages.

In the library, the work of making room for growing series, and of obtaining volumes or parts to complete series that were imperfect, has been continued. In the course of this work the Council have, upon the recommendation of the Library Committee, distributed some 1500 volumes, consisting partly of duplicates and partly of works of small scientific value, among various public institutions. The catalogue of the manuscripts, which I mentioned in my last year's address as about to be commenced, has been completed during the past session. The maps and charts, the pictures and busts, have also been catalogued; and the collection of the manuscripts of memoirs

in the 'Philosophical Transactions,' the 'Proceedings,' and the 'Archives,' having been completed, the binding of them is now going forward.

The Royal Society has always been ready to assist the Government of the day when requested so to do, by expressing its opinions or offering its advice on questions involving special scientific knowledge. Last year I received, as your President, a request from the President of the Board of Trade that I should, in conjunction with two Fellows of the Royal Society nominated by me in consultation with the Council, examine a report in two parts presented by the Corporation of the Trinity House to the Board of Trade, relative to Lighthouse Illuminants, and express our opinion whether the conclusions of the Trinity House, as set forth in their Reports, are justified by the records of the experiments contained therein. Lord Rayleigh and Sir William Thomson were asked, and consented, to join me as referees, and our Report was some time ago sent in to the Board of Trade, and is now in the hands of the public.

Another subject in which scientific principles are blended with practical application, is that of colour blindness in its relation to the correct perception of coloured signals used at sea and in the railway service. It is easy to understand what serious accidents might be occasioned, for instance, by confusing red with green; and so well is the liability to such confusion, arising from a not very rare abnormal condition of colour perception, understood at the present day, that persons who propose to engage in service at sea or on the railways are now, as a matter of course, examined as to their perception of colour. But, glaring as the difference between red and green appears to persons whose vision is normal, the detection of those who are liable to confound them, and who, for the most part, are quite unconscious that they see colours differently from people in general, is by no means so easy as it might appear at first sight; and there appeared reason to think that sometimes the tests applied are defective, and let pass persons who are afflicted with this peculiarity of vision, while, on the other hand, they may lead to the rejection of persons whose vision is normal, perhaps, after they have engaged as they hoped for life in an employment for which normal vision is demanded. Mr. R. Brudenell Carter wrote a letter to us suggesting that we should appoint a committee to investigate the subject of colour blindness, and after discussion of this proposal I was requested to write a letter to the President of the Board of Trade informing him that, should the Government desire it, the Council will be prepared to appoint a committee to consider the whole question of colour blindness. A reply was received from the Board thanking us for the communication, and saying that they regarded with satisfaction the proposal of the Council to appoint such a committee. A committee

has accordingly been appointed, and has held several meetings, and examined several witnesses; but the subject is a wide one, and the committee have not yet brought their labours to a close.

The proceedings of to-day bring to an end my long tenure of office in the Royal Society, which has extended now over thirty-six years, during the last five of which I have held the honourable office of your President. I am deeply sensible of the kindness which I have always experienced from the Fellows, and of the indulgence with which they have overlooked my deficiencies, due, in part, to the pressure of other work. It cannot be without a strong feeling of regret that I come to the close of an official connexion with the Society that has now extended over full half my life. But I feel that it is time that I should make way for others, and that I should not wait for those infirmities which advancing years so often bring in their train; besides which there are personal reasons which led me to request the members of the Council not to vote for my nomination for re-election as your President.

And now it only remains to me, as virtually my last official act as your President, to perform the pleasing duty of delivering the medals which the Society has to award to the respective recipients of those honours.

The Copley Medal has been awarded to our Foreign Member, Professor Simon Newcomb, who has been engaged during the last thirty years in a series of important researches, which have contributed greatly to the progress of gravitational astronomy. Among his labours in this field may be mentioned his able discussion of the mutual relations of the orbits of the Asteroids, with reference to Olbers' hypothesis, that they were formed by the breaking up of a ring of nebulous matter, his discussion of the orbits of Uranus and Neptune, and of the orbit of the Moon. Recently he has turned his attention to Saturn's satellites, and has investigated the remarkable action of Titan on Hyperion. For many years back he has chiefly been engaged in perfecting the tables of the Moon; and in his important work, "Researches on the Motion of the Moon," he has discussed observations of eclipses and occultations previous to 1750, with the important practical result that by the removal of an empirical term of long period from Hansen's lunar tables, and by an empirical alteration of another term of long period, he is enabled to represent satisfactorily the observations of the Moon from 1625 to the present time.

The Rumford Medal has been awarded to Professor H. Hertz for his work on Electro-magnetic Radiation.

One of the most remarkable achievements of the late Professor Clerk Maxwell was his electro-magnetic theory of light, in which it was shown that a certain velocity, determinable numerically by purely

electrical experiments, and expressing theoretically the velocity of propagation of an electro-magnetic disturbance, agreed within the limits of error of experiment with the known velocity of propagation of light; and accordingly that we have strong reason for believing that light is an electro-magnetic phenomenon, whatever the appropriate physical idea may hereafter prove to be which we ought to attach to the propagation of an electro-magnetic disturbance. But as yet no means existed by which phenomena, such as those of interference, which are bound up with the propagation of undulations, could be exhibited by purely electrical means. Professor Hertz was the first to detect electro-magnetic waves in free space by his invention of a suitable receiver, consisting of what may be called a resonating circuit, which gives visible sparks when immersed in a region of sufficiently intense electric radiation.

By reflection, refraction, and interference experiments, he has further verified the undulatory nature of the disturbance near a quick electric oscillator, such as had been suggested by Professor Fitzgerald, on the basis of Clerk Maxwell's electro-magnetic theory of light, and Sir W. Thomson's theory of the oscillatory character of a Leyden jar discharge.

These important researches contribute powerfully to the inducements we have to refer the phenomena of light and electricity to a common cause, different as hitherto their manifestations have been; and by this means the theory of each may be advanced through what we know of the other.

One of the Royal Medals has been awarded to our Fellow, Dr. David Ferrier, for his researches on the localisation of cerebral functions.

We owe to his experiments on monkeys, animals which he was the first to use for this purpose, the beginning and indeed the greater part of our knowledge of cerebral localisation in man. From pathological observations, Broca located the centre for speech in the third left frontal convolution, but with this exception nothing was known of cerebral localisation in man until Dr. Ferrier commenced his experiments in 1873.

Fritsch and Hitzig in 1870 had observed that definite movements could be obtained by electrical irritation of the cerebral cortex in the dog, and this indicated the existence of localised motor areas in the brain. They did nothing, however, towards localising sensory centres, and even in regard to the motor centres their observations were very limited. Their experiments, moreover, were confined to dogs, and it may be doubted whether any great or rapid advance would have been made had not Ferrier hit upon the happy device of experimenting upon monkeys, whose brains present a great similarity in the arrangement of the convolutions to those of man. By employing these

animals, he was able to map out the most important *motor* areas with great precision; but, not content with the investigation of motor centres, he experimented on the localisation of *sensory* centres in the brain, and not only showed the existence of such centres, but determined their position. And if in the numerous observations which have been subsequently made by other inquirers results somewhat differing from his own have been obtained, the main outlines of his conclusions have been confirmed. As so often happens, these researches, purely scientific in the first instance, have been turned to practical account. Dr. Ferrier himself predicted the application of cerebral localisation to cerebral surgery. This application others have already made, and his prediction is now being fulfilled with brilliant results.

The other Royal Medal has been awarded to our Fellow, Dr. John Hopkinson, for his researches in magnetism and electricity.

Dr. Hopkinson's researches in magnetism comprise investigations of the effect of temperature upon the magnetic properties of iron, nickel, and various alloys of these metals ('Phil. Trans.,' 1889, A, p. 443). It was known that for small magnetising forces the magnetisation of iron, nickel, and cobalt increases with increase of temperature until we approach a certain temperature, which may be called the critical temperature, on passing which the magnetism almost suddenly disappears. Dr. Hopkinson's experiments show that for small magnetising forces not only does the magnetisation increase with rise of temperature, but on approaching the critical temperature the increase becomes extremely rapid, and then, on still further increase of temperature, the magnetism suddenly almost entirely disappears. He has further determined the critical temperature for various samples of iron and steel. He has also measured the rapid change which takes place in the temperature coefficient of electrical resistance in the neighbourhood of the critical temperature, and has shown that, as had been conjectured, it is really at the critical temperature that the phenomenon of recalescence takes place.

Dr. Hopkinson's contributions to the theory of dynamo-electric machinery are most important. The method, now so extensively used, of solving problems relating to dynamos by the use of what M. Deprez has called the "characteristic curve," is due to him.

He has also made a series of determinations of the specific inductive capacities and refractive indices of a large number of transparent dielectrics, the results of which are of great importance in the theories of electricity and light.

The Davy Medal has been awarded to Professor Emil Fischer for his discoveries in organic chemistry, and especially for his researches on the carbo-hydrates.

To him, in conjunction with Otto Fischer, we owe the determina-

tion of the constitution of rosaniline, a most valuable dye-stuff, and the typical member of a very large group of important dyes.

He is the discoverer of phenylhydrazine, one of the most important of the reagents placed at chemists' disposal within recent years, and he has most exhaustively studied the behaviour of this substance and its congeners. The hydrazines have also been employed by Fischer in preparing indole derivatives, among others, skatole, and the study of a class of substances of considerable physiological importance has thereby been rendered possible.

During the past seven years Fischer has devoted his attention to the study of the sugars, and has obtained most marvellous results, having succeeded in preparing, by purely artificial methods, the known sugars dextrose and levulose, as well as other isomeric sugars, and having established the relationship of the various members of the glucose group. He has, in addition, determined the constitution of milk-sugar and of starch-sugar—the isomer of cane-sugar formed on hydrolysing starch. He has also prepared "glucoses" containing seven, eight, and nine atoms of carbon, and has established the remarkable fact that only those which contain three, six, or nine atoms of carbon are fermentable by yeast. His researches are not only of the highest value to chemists, but also of extreme importance to physiologists, on account of the insight which they promise into the processes concerned in the natural formation of sugars.

The Darwin Medal has been awarded to Mr. Alfred Russel Wallace for his independent origination of the theory of the origin of species by natural selection.

It was natural that this, the first, award of the Darwin Medal should have been made to one who independently originated the theory, since named that of natural selection, which, in conjunction with his other numerous and important contributions in the domain of natural history, has made the name of Darwin so famous, and who made known a large series of important and novel observations in support of that theory, the result of many years' work in the Malay Archipelago. These views Mr. Wallace has subsequently most ably advocated in various published works, among others his laborious volumes on the 'Geographical Distribution of Animals,' his brilliant 'Island Life,' and more recently his 'Darwinism,' which was published only last year.

The Statutes relating to the election of Council and Officers were then read, and Mr. Common and Mr. Symons having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Sir William Thomson, D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Archibald Geikie, LL.D.

Other Members of the Council.

Professor William Edward Ayrton; William Henry Mahoney Christie, M.A.; Professor W. Boyd Dawkins, M.A.; James Whitbread Lee Glaisher, D.Sc.; Hugo Müller, Ph.D.; Professor Alfred Newton, M.A.; Sir William Roberts, M.D.; William Chandler Roberts-Austen, F.C.S.; Professor Edward Albert Schäfer, M.R.C.S.; Sir George Gabriel Stokes, Bart, M.A.; Lieut.-General Richard Strachey, R.E.; Professor Joseph John Thomson, M.A.; Professor Thomas Edward Thorpe, B.Sc.; Sir William Turner, M.D.; Professor Sydney Howard Vines, M.A.; General James Thomas Walker, C.B.

The thanks of the Society were given to the Scrutators.

Balance Sheet. 1890.

Statement of Receipts and Expenditure from November 12th, 1889, *to November* 12th, 1890.

Receipts	£	s.	d.	£	s.	d.
To Balance at Bank, 12th November, 1889				94	7	3
,, Balance in hand, Catalogue Account	2	17	7	10	11	11
,, ,, Petty Cash	7	14	4			
,, Annual Contributions, 148 at £4	592	0	0	1,045	0	0
,, 151 at £3	453	0	0			
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				301	0	0
,, Rents:						
Fee Farm, Lewes	18	14	5	103	14	5
Mablethorpe Estate	85	0	0			
,, Ground Rents				588	18	9
,, Dividends (exclusive of Trust Funds)				2,032	9	9
,, Interest on Mortgage Loan				585	0	0
,, Sale of Transactions and Proceedings				653	18	6
,, Sale of Catalogue				5	5	0
,, Sale of Krakatoa Report (leaving £94 17*s.* 3*d.* Expenditure in excess of Receipts)				8	5	0
,, Transfer from Handley Fund on account of Catalogue				187	2	4
,, Compositions				180	0	0
,, Lendenfeld Monograph, Sales	£10	5	0	210	5	0
,, Contribution from Australian Museum, Sydney	200	0	0			
,, Challenger Report, Government Grant				1,600	0	0
,, Law Costs refunded *re* Lawes Trust				59	11	11
				£7,665	9	10

Expenditure	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,755	12	0
,, Catalogue of Scientific Papers				156	11	0
,, Books for the Library				245	7	7
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	183	10	1	2,196	16	2
,, Ditto Proceedings, Nos. 280 to 294	826	13	3			
,, Ditto Miscellaneous	115	19	0			
,, Paper for Transactions and Proceedings	344	5	9			
,, Binding ditto	43	14	0			
,, Engraving and Lithography	682	14	1			
,, Soirée and Reception Expenses				161	7	2
,, Coal, Lighting, &c.	185	1	9	648	12	6
,, Office Expenses	45	0	1			
,, House Expenses	186	11	5			
,, Tea Expenses	19	13	8			
,, Fire Insurance	55	5	0			
,, Taxes	44	12	6			
,, Advertising	17	17	0			
,, Postage, Parcels, and Petty Charges	64	10	0			
,, Miscellaneous Expenses	30	1	1			
,, Lendenfeld Monograph (making with previous Expenditure £682 6*s.* 9*d.* in excess of Receipts)				16	9	2
,, Krakatoa Report				2	10	0
,, Carrington Donation				37	10	0
,, Challenger Report, payment to Mr. J. Murray				600	0	0
,, Electric Lighting, Plant, &c.				65	6	4
,, Balance at Bankers				1,753	9	5
,, Balance on hand, Catalogue Account	£2	15	1	25	18	6
,, Ditto, Petty Cash	23	3	5			
				£7,665	9	10

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1889:—						
General Account	1,171	18	7			
Fee Reduction Fund Account	198	18	8	2,141	10	7
Scientific Relief Fund Account	770	13	4			
„ Scientific Relief Fund, Dividends, and Donations	653	12	2			
„ Donation Fund, Dividends, and Transfer from Jodrell Fund	389	11	6			
„ Rumford Fund, Dividends	62	8	1			
„ Bakerian and Copley Medal Fund, Dividends, &c.	54	10	0			
„ Keck Bequest, Dividends	23	8	0			
„ Wintringham Fund, Dividends, &c.	32	4	0			
„ Croonian Lecture Fund—Rent	50	9	8	4,875	1	0
„ Davy Medal Fund, Dividends	32	3	6			
„ Gassiot Trust, Dividends, &c.	967	13	5			
„ Handley Fund, Dividends	187	2	4			
„ Jodrell Fund, Dividends	138	19	4			
„ Fee Reduction Fund, Dividends	421	8	11			
„ Darwin Memorial Fund, Dividends	85	16	0			
„ Joule Memorial Fund, Subscriptions and Dividends	1,775	14	1			
				£7,016	11	7

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Grants	692	0	0			
„ Donation Fund, Grants	150	10	0			
„ Rumford Fund, Purchase of Consols	6	18	2			
„ Bakerian and Copley Medal Fund, Medal, and Gift	54	12	0			
„ Keck Bequest, Payment to Foreign Secretary	23	8	0			
„ Wintringham Fund, Payment to Foundling Hospital	42	8	6			
„ Croonian Lecture Fund, Payment	50	9	8			
„ Davy Medal Fund, Gold Medals	32	4	6			
„ Gassiot Trust, Payments to Kew Committee	487	10	0	4,457	7	8
„ Ditto, Investment	442	10	6			
„ Handley Fund, Transfer to General Account	187	2	4			
„ Jodrell Fund, Transfer to Donation Fund	138	19	4			
„ Fee Reduction Fund, Investment	225	15	6			
„ Ditto, Transfer to Royal Society General Account (1890)	301	0	0			
„ Darwin Memorial Fund, Expenses	0	9	0			
„ Joule Memorial Fund, Payments and Investments	1,621	10	2			
„ Balance at Bankers:—						
General Account	1,579	2	5			
Fee Reduction Fund Account	93	12	1	2,559	3	11
Scientific Relief Fund Account	732	5	6			
Joule Memorial Fund Account	154	3	11			
				£7,016	11	7

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £100 per annum.

Ground Rent of House, No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Bank Stock and other Securities (produced £661 9*s*. 5*d*. in 1889–90).

The Funds in Court now standing to the credit of the cause are as follows:—

- £11,000 Bank Stock.
- £11,031 London and North Western Railway Consolidated 4 per Cent. Guaranteed Stock.
- £11,105 Great Northern Railway 4 per Cent. Perpetual Preference Stock.
- £11,031 North Eastern Railway Consolidated 4 per Cent. Guaranteed Stock.
- £8,894 Great Western Railway 5 per Cent. Consolidated Guaranteed Stock.
- £11,035 16*s*. 5*d*. Midland Railway 4 per Cent. Consolidated Preference Stock.

Subject to certain charges, the Royal Society is entitled to one-fourth of the proceeds.

£15,000 Mortgage Loan, 4 per Cent., about to be paid off.

£14,297 8*s*. 5*d*., $2\frac{3}{4}$ per Cent. Consolidated Stock, being £10,779 8*s*. 2*d*. on account of the following Funds:—

	£	s.	d.
Rumford Fund	2,330	0	0
Wintringham Fund	1,200	0	0
Gassiot Trust	400	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

and £3,518 0*s*. 3*d*. in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent. Stock.—Bakerian and Copley Medal Fund.

£1,000 India $3\frac{1}{2}$ per Cent. Stock.—General Purposes.

£800 Midland Railway 3 per Cent. Debenture Stock.—Keck Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000. Davy Medal Fund, £660.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£6,396 Great Northern Railway 4 per Cent. Debenture Stock { Scientific Relief Fund, £5,000. The Trevelyan Bequest, £1,396.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.

£4,400 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£18,150 ,, ,, ,, ,, 4 per Cent. Consolidated Guaranteed Stock.—{ £6,000 Scientific Relief Fund. £12,150 General Purposes.

£5,000 London and North Western Railway Consolidated 4 per Cent. Preference Stock.—General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Memorial Fund.

£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.

£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.

£1,000 Policy in the Atlas Assurance Office, becoming due October 7th, 1899.—Catalogue Account.

£800 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.—Joule Memorial Fund.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of Council, have examined these Accounts and found them correct.

HUGO MÜLLER.
EDWARD J. ROUTH.

We, the Auditors of the Treasurer's Accounts on the part of the Society have examined these Accounts and found them correct.

JAMES COCKLE.
A. AINSLIE COMMON.
G. CAREY FOSTER.
JOHN RAE.

Trust Funds. 1890.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£5,000 Great Northern Railway 4 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.

Dr.	£	s.	d.	£	s.	d.	*Cr.*				£	s.	d.
To Balance { Income	801	4	7				By Grants				692	0	0
Less — Capital over-invested	30	11	3	770	13	4	„ Balance, Income	£756	15	9	732	5	6
„ Dividends				640	11	6	„ Less—Capital over-invested	24	10	3			
„ Annual Subscriptions				6	1	0							
„ Interest on Deposit				6	19	8							
				£1,424	5	6					£1,424	5	6

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,396 Great Northern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	550	10	11	By Grants	150	10	0
„ Dividends	250	12	2	„ Balance	789	12	5
„ Transfer from Jodrell Fund	138	19	4				
	£940	2	5		£940	2	5

Rumford Fund.

£2,330 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	87	8	2	By Purchase of £7 1*s.* 0*d.* $2\frac{3}{4}$ per Cent. Stock	6	18	2
„ Dividends	62	8	1	„ Balance	142	18	1
	£149	16	3		£149	16	3

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* $2\frac{3}{4}$ per Cent. Consolidated Stock.
£403 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	119	4	6	By Gold Medal	4	12	0
„ Dividends, New $2\frac{1}{2}$ per Cent. Stock	9	16	8	„ Rev. Dr. Salmon—Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund	44	13	4	„ Balance	119	2	6
	£173	14	6		£173	14	6

The Keck Bequest.

£800 Midland Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends	23	8	0	By Payment to Foreign Secretary	23	8	0

Wintringham Fund.

£1,200 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	42	8	6	By Payment to Foundling Hospital	42	8	6
„ Dividends	32	4	0	„ Balance	32	4	0
	£74	12	6		£74	12	6

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Rent	50	9	8	By Lecture—Dr. E. Roux	50	9	8

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	77	0	1	By Gold Medals	32	4	6
„ Dividends	32	3	6	„ Balance	76	19	1
	£109	3	7		£109	3	7

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

£400 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	14	8	11	By Payments to Kew Committee	487	10	0
" Dividends	498	4	8	" Purchase of £400 Italian Irrigation Bonds	442	10	6
" £400 Italian Irrigation Bonds drawn	469	8	9	" Balance	52	1	10
	£982	2	4		£982	2	4

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends	187	2	4	By Transfer to Catalogue Account	187	2	4

The Jodrell Fund.

£5,182 14*s.* 10*d.* 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	138	19	4	By Transfer to Donation Fund	138	19	4

Fee Reduction Fund.

£4,400 Metropolitan 3½ per Cent. Stock.
£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	198	18	8	By Transfer to Royal Society General Account	301	0	0
„ Dividends	421	8	11	„ Purchase of £200 Metropolitan 3½ per Cent. Stock	225	15	6
				„ Balance	93	12	1
	£620	7	7		£620	7	7

Darwin Memorial Fund.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	280	17	6	By Printing	0	9	0
„ Dividends	85	16	0	„ Balance	366	4	6
	£366	13	6		£366	13	6

Joule Memorial Fund.

£800 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Subscriptions	1,754	7	3	By Purchase of £800 L. B. & S. C. R. Stock	1,278	19	6
„ Interest on Deposit	1	16	10	„ Expenses	42	10	8
„ Dividend	19	10	0	„ On Deposit on behalf of the Committee	800	0	0
				„ Balance	154	3	11
	£1,775	14	1		£1,775	14	1

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1889 ..	5	47	176	151	139	518
Since Elected ..	..	+ 3	+ 3	..	+ 12	+ 18
Since Deceased ..	..	− 1	− 11	− 5	− 1	− 18
Defaulter ..	..	..	..	− 1	..	− 1
Dec. 1, 1890 ..	5	49	168	145	150	517

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XLVI, p. 469).

1889–1890.

	£
E. Nevill, for continuing his Deduction of the Corrections to the Theory embodied in Hansen's Lunar Tables	50
P. Smyth, for further Researches in Spectroscopic Measurement of Ultra Definition and Extreme Separation	100
J. N. Lockyer, for Astrophysical Observations, including Long Exposure Photographs of the Spectra of Nebulæ with Comparison Spectra	125
G. Higgs, for the Production by means of Photography of a Map of a Normal Solar Spectrum from w. l. 3,000 to 10,000..	50
C. Davison, for the Preparation of the Bibliography of Seismology (1800–83)	10
A. M. Worthington, for an Investigation of the Relation between Tensional Stress and Strain in Liquids	40
Dr. J. W. Tripe, for further Aid in Providing Assistance for the Discussion of Observation of Thunderstorms in 1888–89	30
Prof. W. Ramsay, for an Investigation of the Ratio between the Specific Heat at Constant Volume and at Constant Pressure of (primarily) Ether-gas	60
Carried forward	£465

	£
Brought forward	465
J. I. Plummer, for Reducing and Publishing Observations of Eight Comets and for further Observations	50
Profs. Rücker and Thorpe, for a further Magnetic Survey of the United Kingdom	600
Colour-vision Committee of the Royal Society	100
J. B. Cohen, for a Research on the Constitution of the Compounds of Ammonia with Sulphuretted Hydrogen	50
W. H. Perkin, jun., for further Research on the Constitution of the Alkaloid Berberin................................	50
F. R. Japp, for an Investigation of the Reactions of Ketones, Diketones, and Allied Compounds	75
G. J. Burch and J. E. Marsh, for Continuation of their Researches on Amine Vapours..............................	50
A. Smith, for a Research on the Synthesis and Properties of 1 : 3- and 1 : 4-Diketones..............................	10
S. Pickering, for an Investigation on the Nature of Solutions, and the Law of the Freezing Points of Solutions............	50
Dr. F. S. Kipping, for an Investigation of the Action of Phosphoric Anhydride on Fatty Acids	25
A. P. Laurie, for Continuation of Researches on the Composition of Alloys	40
Prof. W. R. Dunstan, for further Investigation of the Connexion between the Chemical Constitution of certain Organic Nitrites and their Physiological Action....................	50
Dr. C. R. A. Wright, for further Researches on Ternary Alloys	50
T. R. Marshall, for a Research on the Constitution of Diacetyl Tetramethylene Carboxylic Acid, and three other Researches	15
S. Ruhemann, for a Research on Mucic and Saccharic Acids	20
G. S. Turpin, for a Research on the Ignition of Explosive Gaseous Mixtures	50
H. Gordon, for an Investigation of the Question whether Direct Isomeric Change of Meta- into Para-compounds, or of Ortho- into Meta-compounds, is possible in the Benzine Series	20
Prof. Armstrong, for the Study of Naphthalene Derivatives with the object of determining the Laws which govern the Formation of Substitution Derivatives of Benzenoid Hydrocarbons	100
P. J. Hartog, for the Completion of a Thermochemical Research on the Sulphides	50
Carried forward........	£1,920

	£
Brought forward	1,920
W. C. Williamson, for further Research on the Organisation of the Fossil Plants of the Coal-measures	50
R. Kidston, for a Research on the Vertical and Horizontal Distribution of British Palæozoic Flora	40
Scientific Committee, Royal Horticultural Society (per D. Morris), for an Inquiry into the Composition of London Fog, with special regard to the Constituents of Fog injurious to Plant-life	100
Dr. G. H. Fowler, for Researches connected with the Bionomics of certain Lower Crustacea	150
Prof. J. R. Green, for Continuation of Researches into the Processes of Germination in Plants	30
T. Johnson, for further Investigation of Obscure or Unknown Points in the Florideæ	25
F. E. Beddard, for a Research on the Structure of the Oligochæta	50
E. R. Lankester, for further Investigation of Larval Development of Amphioxus	100
G. Massee, to prepare a Monograph of the Myxogastres, and to investigate the Life-history of Typical Members of the Group	50
Liverpool Marine Biological Committee (per Prof. Herdman), for the further Exploration of the Fauna and Flora of Liverpool Bay	50
W. Heape, for an Investigation of the Phenomena of Menstruation and Ovulation, and of the Early Stages of Development of the Monkey	100
West Indies Committee (per D. Morris), for further Aid in Collecting and Describing Fauna and Flora in the less known West India Islands	100
R. Irvine, for a Research on (1) the Solvent Power of Seawater upon Carbonate of Lime; (2) the Secretion of Silica...	100
J. T. Cunningham, for a Month in Norway to get Developing Eggs of *Myxine glutinosa*	40
E. A. Schäfer, for Continued Investigations into the Functions of the Brain of Monkeys	100
W. Hale White, for further Investigation of the Effect upon Bodily Temperature of Lesions of the Corpus Striatum and Optic Thalamus	50
Prof. P. F. Frankland, for further Study of the Chemical Changes induced by Specific Micro-organisms	50
Carried forward	£3,105

	£
Brought forward..........	3,105
W. MacLennan, for an Experimental Investigation into all the Known and Isolated Alkaloids of Opium................	15
W. D. Halliburton, for further Researches on the Chemical Physiology of the Animal Tissues.........................	50
Dr. J. R. Bradford, for a further Investigation into the Functions of the Kidney	75
Dr. Lingard, for further Researches as to the Relation existing between the Fœtus and its Mother	40
Drs. Gulland, Edington, and Ritchie, for the Purchase of two High-power Objectives for Research Purposes..........	50
Prof. Tait, for Aid in an Investigation on the Duration of Impact..	30
W. T. Thiselton Dyer, to enable Mr. Gustav Mann to accompany, as Botanist, the Delimitation Commission in Sierra Leone	100
J. B. Farmer, to enable him to Proceed to Ceylon to Study the Morphology of the Hepaticæ.........................	100
	£3,565

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance, November 30, 1889 .	61	2	4	By Appropriations, as above.............	3,565	0	0
,, Grant from Treasury	4,000	0	0	,, Salaries, Printing, Postage, Advertising, and other Administrative Expenses...........	104	7	3
,, Repayments	33	1	9	,, Balance, Nov. 30, 1890	460	18	8
,, Interest on Deposit.........	36	1	10				
	£4,130	5	11		£4,130	5	11

Account of Grants from the Donation Fund in 1889–90.

	£	s.	d.
Prof. Schäfer, to assist Messrs. Bradford and Dean in a Research on the Vasomotor Nerves of the Pulmonary Vessels	15	0	0
Prof. G. H. Darwin, towards the Payment of Computers for the Reduction of his Tidal Observations	25	0	0
The Astronomer Royal, to assist Mr. Downing in Computing the Orbit of the Minor Planet Juno	10	10	0
G. J. Symons, in Aid of Expenses connected with the construction of his Brontometer	100	0	0
	£150	10	0

Report of the Kew Committee for the Year ending October 31, 1890.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.	The Earl of Rosse, K.P.
Prof. W. G. Adams.	Prof. A. W. Rücker.
Staff-Commander E. W. Creak, R.N.	Mr. R. H. Scott.
Prof. G. C. Foster.	Lieutenant-General R. Strachey, C.S.I.
Admiral Sir G. H. Richards, K.C.B.	General J. T. Walker, C.B.
	Captain W. J. L. Wharton, R.N.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. Magnetic Observations.

Throughout the past year the magnetographs have worked in a satisfactory manner. In accordance with the usual practice, determinations of the scale values of all the instruments were made in January last.

The values of the ordinates of the different photographic curves then determined, were as follows:—

Declinometer: 1 inch = 0° 22′·04. 1 cm. = 0° 8′·7.

Bifilar, January 10, 1890, for 1 inch δH = 0·0278 foot grain unit.
 " 1 cm. " = 0·00050 C.G.S. unit.

Balance, January 13, 1890 for 1 inch $\delta V =$ 0·0296 foot grain unit.
„ 1 cm. „ = 0·00054 C.G.S. unit.

In the case of the vertical force magnetometer, it was found necessary to re-adjust the instrument, and as at the same time its sensibility was slightly altered, the scale value was again determined, with the following result:—

Balance, January 21, 1890, for 1 inch $\delta V =$ 0·0284 foot grain unit.
„ 1 cm. „ = 0·00051 C.G.S. unit.

With regard to magnetic disturbances, no very exceptional movements have been recorded during the year.

The principal disturbances were on the following dates:—November 1 and 26—28, 1889.

The monthly observations of Horizontal Force, Inclination, and Declination with the absolute instruments have been made in accordance with the usual practice.

Information on matters relating to terrestrial magnetism and various data have been supplied to Professors Thorpe and Rücker, Dr. van Rijckevorsel, Dr. Atkinson, Professor Chistoni, and Captain Schück.

Magnetic Sub-Committee.—Professors W. G. Adams, A. W. Rücker and Captain Creak having been requested by the Committee to act as a sub-committee to consider the form to be employed in framing the appendices to this Report, have held two meetings. They decided that in future the Magnetic and Meteorological Appendices should be presented to the Royal Society as soon after the 1st of January as possible, instead of accompanying the Report itself, and therefore closing with September 30th as heretofore. This arrangement will take effect with the present issue.

At the suggestion of Professor Rücker, the Kew Magnetical Declination results for the years 1883, 1886, and 1887 have been recently discussed in a paper published by the Physical Society of London in their Proceedings, entitled "On the Diurnal Variation of the Magnet at Kew," by Messrs. Robson and Smith. These gentlemen, students in the physics class at the Royal College of Science, kindly gave their assistance in the labour of tabulation and computation.

At the request of Professors Thorpe and Rücker, facilities have been afforded to Messrs. Gray and Briscoe, who are also attached to the physical laboratory in the Royal College of Science, South Kensington, to make magnetic base observations from time to time at Kew, with the view of their employment in the Magnetic Survey of Great Britain and Ireland now in progress on a more extended scale than that on which it has before been carried out.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of Atmospheric Pressure, Temperature, and Humidity, Wind (direction and velocity), Bright Sunshine, and Rain have been maintained in regular operation throughout the year.

The standard eye observations for the control of the automatic records have been duly registered, together with the daily observations in connexion with the U.S. Signal Service synchronous system.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine, have been transmitted to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the editor of 'Symons's Monthly Meteorological Magazine,' Dr. Rowland, and others.

Tables of the monthly values of the rainfall and temperature have been regularly sent to the Meteorological Sub-Committee of the Croydon Microscopical and Natural History Club for publication in their Proceedings. Detailed information of all thunderstorms observed in the neighbourhood during the year has been forwarded to the Royal Meteorological Society, soon after their occurrence.

Electrograph.—This instrument has been in constant action throughout the year, and comparisons with the portable electrometer have been made from time to time.

'*Times*' *Weather Chart.*—The supply of the chart exhibiting copies of the daily traces of the self-recording meteorological instruments at the Observatory ceased by instructions from the 'Times' office in March last, after continuous publication for 14 years.

The fog gauge set up on the north side of the Observatory in 1884 has been recently dismounted, as it has not been found possible to measure the intensity of this phenomenon by its means.

Fort William Observatory.—At the request of the Meteorological Council, a barograph and a thermograph which have been stored at Kew Observatory since their return from Armagh Observatory in 1886 have been thoroughly re-fitted, and, after a short experimental trial, were re-packed and forwarded to the new Observatory at Fort William for use at the low-level station worked in conjunction with the Observatory erected on the summit of Ben Nevis.

In June last, on receipt of information from Mr. Omond, the superintendent of the Ben Nevis Observatory, that the new building was ready for the reception of the instruments, Mr. T. W. Baker proceeded to Fort William and set them up and put them in proper adjustment. Having done this, and instructed Mr. Omond in their manipulation

and the attendant photographic operations, he returned to Kew, leaving the establishment in good working order on July 14.

Owing to the cost of gas, mineral oil is used as the illuminant, as is the case at Valencia Observatory also.

III. Solar Observations.

Sketches of Sun-spots have been made on 198 days, and the groups numbered after Schwabe's method.

Time Signals.—These have been received with great regularity all through the year, failure in transmission having only occurred on six days, on one of which the signal was duly received at the proper time, but was not recorded, the chronograph clock having been deranged by an accident.

Transit Observations.—Solar and sidereal transits have been occasionally observed as a check on the signalled times.

During the past summer 225 series of observations of the Sun's actinic power have been made with Violle's actinometer, described in the last Annual Report, upon the plan arranged by General Strachey and Mr. Blanford. Copies of the instrumental readings will be transmitted to the Meteorological Office for discussion, the cost of the experiments being defrayed by that establishment.

By the kindness of Mr. C. Baker, of High Holborn, the Observatory has received the original sunshine record cards obtained by the late Mr. Rand Capron, F.R.A.S., at his observatory near Guildford, Surrey, during the years 1880 to 1887.

The Winstanley radiograph, deposited at the Observatory in 1880, was recently repaired at the suggestion of Mr. R. H. Scott, and set up on the lawn. Its action is, however, not considered satisfactory, and it has been decided to return it to the owner.

IV. Experimental Work.

The *Electrical Anemograph*, after working on the staging erected on the roof, 14 feet to the north of the Beckley instrument, and recording by means of a battery composed of eighteen Fuller's cells, was dismounted on July 22, and packed for storage. During the period it was at work, the traces were forwarded weekly to the Meteorological Office.

Oils.—At the request of the Meteorological Office, various specimens of lubricating oils have been applied to the gearing of the anemograph with the view of determining the best for use under the varying conditions to which it is exposed.

Pendulums.—The swinging of the invariable pendulums at the Royal Observatory, Greenwich, having been completed during the past winter, the apparatus was dismounted and returned to Kew,

where it is now stored awaiting further disposal. A paper describing the operations, and giving full details of the values of the vibration numbers obtained at Kew and at Greenwich, has been contributed to the Royal Society by General Walker (see 'Phil. Trans.,' vol. 181, A, p. 537). The result shows the number of vibrations made by a seconds' pendulum in one day to be 0·64 greater at Kew than at Greenwich.

Cloud Photographs.—At the suggestion of General Strachey, Chairman of the Meteorological Council, a new departure has been made in the photography of clouds during the past year, with the view of simplifying the operations of determining the height and velocity of their movement. Both cameras have been rigidly fixed on their stands, with the axes of their lenses pointed directly to the zenith, and photographs are now taken simultaneously of the area of the sky surrounding the zenith within a circle of a radius of about 15°. These photographs are superposed one on the other, so that the two pictures shall appear to coincide, and a simple measurement of the distance between the images of the zenith points, which are marked by intersecting lines, gives a means of readily determining the height of the cloud above the surface of the ground. A second measurement made in like manner of the displacement of the zeniths in a second pair of photographs taken after a given interval of time serves to show the rate of travel of the cloud and the direction in which it is moving at the instant of observation. Twenty groups of clouds, giving heights extending from $1\frac{1}{2}$ miles to 8 miles, and rates of motion from 5 miles to 64 miles per hour, have been photographed and measured in this manner during the past summer.

A wooden frame, 12 feet in height, has been constructed, which is occasionally erected above each of the cameras in order to verify the position of their zenith points and the orientation of the cross lines on the photographic plates.

V. Verification of Instruments.

The following magnetic instruments have been purchased on commission and their constants determined:—

1 Inclinometer, for Padre Denza, Rome.
2 Collimator magnets for Professor Chistoni, Italy.
1 Pair of dip needles for Hong Kong Observatory.
1 Pair of small dip needles for Senhor Capello.
A single dip needle repaired for Professor Mielberg.

Sextant Testing.—This branch of the work of the Verification Department has been very active during the past year, 346 instruments having been examined and certified, and tables of corrections supplied. Owing to the decay of the photographed scales of some of

the collimators through damp, it was thought advisable to fit new and improved scales to all of them. On being furnished with the necessary drawings, Captain Abney kindly had the set of photographs made in his laboratory, and they were duly fitted to the collimators by Mr. Adie.

Care was taken to replace them exactly in the same positions as those occupied by the old scales, and after they were set up a re-determination of all the angles was very accurately made and recorded for future use.

Steps have recently been taken with a view of fitting electric lamps, worked by a storage battery, to the instrument for testing the parallelism of the dark shades, in order that this operation may be performed in the absence of brilliant sunshine, a condition which has hitherto rendered it impossible to complete the examination of these shades in cloudy weather.

Sextant Telescopes.—The Committee, having had their attention drawn to the low optical power of some of the telescopes supplied with sextants submitted to them for examination, have given instructions that no certificate of the highest class should be issued with any instrument if the telescope is not capable of distinguishing the smallest angle exhibited by the division of the graduations on the arc of the sextant.

The *Hydraulic Press* used for testing the behaviour of deep sea thermometers under hydrostatic pressures of $3\frac{1}{2}$ tons per square inch is not capable of exerting the higher pressures now required by the Admiralty for thermometers employed in the deeper soundings.

The question of strengthening the press was submitted to Messrs. Armstrong and Co., who reported that the cost of doing so would exceed the sum the Committee could afford to expend upon the apparatus.

Normal Thermometers.—The Committee having considered the desirability of having some thermometers accurately compared with the hydrogen thermometer of the Conservatoire des Poids et Mesures, at Paris, instructed Mr. Whipple to convey to the director of that office the set of three closely graduated mercurial thermometers, whose calibration errors were investigated in 1879, by Professors T. E. Thorpe and Rücker (see 'British Association Report,' 1881, p. 540), and also an alcohol thermometer graduated at Kew for the special purpose of the comparison, its scale extending from $-100°$ to $+90°$ Faht. The examination of these thermometers has now been completed, and M. Benoit has sent his report upon them to Kew Observatory.

In addition to the usual instruments submitted for verification, the Committee have been called upon for special examination and reports referring to the following articles: the Admiralty, for a Gun Director

Telescope, and new pattern Officer's Telescope; the War Office for a barometer supplied to the Netley Hospital; and the makers for a new Watkin's Clinometer, and Watkin's Aneroid with open scales; as well as various instruments for the Anglo-German Boundary Commission on the Gold Coast.

List of Fees.—The Chairman of the Committee, with a view of making the public more conversant with the systems of verification and rating in use at the Observatory, prepared in the early part of the year a pamphlet entitled "Tests and Certificates of the Kew Observatory." Of these 1,000 copies were printed, of which 200 have been distributed to the principal opticians and instrument makers, and others sold to the general public.

The total number of other instruments compared in the past year was as follows:—

Air-meters	5
Anemometers	14
Aneroids	62
Artificial horizons	3
Barometers, Marine	134
" Standard	44
" Station	28
Binoculars	336
Compasses	17
Hydrometers	364
Inclinometers	1
Magnets	2
Navy Telescopes	152
Rain Gauges	15
Rain Measures	33
Sextants	346
" Shades	78
Sunshine Recorders	3
Theodolites	5
Thermometers, Arctic	71
" Avitreous or Immisch's	346
" Chemical	63
" Clinical	12536
" Deep sea	40
" Meteorological	4901
" Mountain	24
" Solar radiation	44
" Standards	100
Unifilars	3
Total	19770

Duplicate copies of corrections have been supplied in 63 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	23
„ ordinary meteorological	20
Various	150

3 Standard Thermometers have also been calibrated, and supplied to 3 applicants during the year.

There are at present in the Observatory undergoing verification, 4 Barometers, 777 Thermometers, 51 Hydrometers, 14 Sextants, and 42 Telescopes.

The increase in the number of sextants verified during the past year has been considerable, 346 instruments of that kind having been tested, whereas the greatest number in any previous year has been 292.

VI. Rating of Watches.

During the year 513 entries of watches for rating were made. They were sent for testing in the following classes:—

For class A, 450; class B, 49; and class C, 9; subsidiary trial, 5.

Of these 128 failed to gain any award; 10 passed with C, 41 with B, 329 with A certificates, and 34 of the latter obtained the highest, class A *especially good.*

In the Appendix will be found statements giving the results of trial of the 26 watches which obtained the highest numbers of marks during the year, the highest position being attained by Mr. A. E. Fridlander, of Coventry. His watch was a keyless double roller with going barrel, which obtained 86·1 marks out of a possible 100.

At the request of several watch makers, the Committee have slightly modified the regulation for the granting of certificates for watches which have been rated. The chief alteration is in the conditions requisite for affixing the words *especially good* to a Class A certificate. These are now simplified so that all watches which have 80 marks and upwards awarded to them after trial are entitled to be characterised as *especially good.*

Marine Chronometers.—Certificates showing the mean daily rate and the variations of rate at three different temperatures have been awarded to 3 marine chronometers after undergoing the 35 days' trial.

An Astronomical Regulator for the Observatory at Akassa, Royal Niger Company's Territory, has also been rated at temperatures of 60° and 80° Faht.

VII. Miscellaneous.

Plans have been prepared and estimates obtained for the construction of the necessary apparatus to enable the examination of photographic lenses for cameras to be prosecuted at the Observatory, with the view of granting certificates to the owners or purchasers of such articles. It is in contemplation to adopt a system of examination of lenses, which shall provide, first, for a comparatively rough or cursory trial which will enable a person to form a general idea of the capabilities of a lens, and, second, for a more lengthy and careful trial, for which a higher fee will be charged, which will give full particulars as to the various qualities an acquaintance with which is necessary to possess a full knowledge of the instrument. Captain Abney and other gentlemen have rendered the Committee much assistance in the practical arrangement of the details of this lens testing.

Toronto University.—An appeal having been received from the librarian of this institution, recently destroyed by fire, for books to replace those lost, the Committee forwarded a parcel of such duplicates as they could spare from the Observatory library, which has been duly acknowledged by the President of the College.

On the occasion of the eleventh annual exhibition of the Royal Meteorological Society, which was devoted to illustrations of the applications of Photography to Meteorology, several instruments and photographs were exhibited, and Mr. Whipple read a paper on the subject, illustrating it by means of the optical lantern.

Building, &c.—A new window has been fitted to light the staircase leading into the Dome, a new stove fitted to the Library in place of the gas stove, and Fletcher's hot water heaters placed in both East and West Rooms for use in verification operations.

The West Room, Library, and Superintendent's rooms have been painted and ceilings whitened.

Prepared photographic paper has been procured and supplied to the Observatories at Aberdeen, Batavia, Colába, Falmouth, Lisbon, Toronto, Oxford, Mauritius, St. Petersburg, and Stonyhurst, as well as to the Meteorological Office for Valencia and Fort William.

Anemograph sheets have been sent to Mauritius and Madras, and blank forms for entry of observations, &c., distributed to various applicants.

Library.—During the year the library has received as presents the publications of—

25 Scientific Societies and Institutions of Great Britain and Ireland, and

118 Foreign and Colonial Scientific Establishments, as well as of numerous private individuals;

The Librarian is still engaged in the preparation of a card catalogue of the library, on the model of that of the Meteorological Office, and has now completed over 1,400 cards, which contain the titles, &c., of all works received by the Committee during the past eight years, together with those of a like title which had been received previously.

The publications not yet catalogued formed part of Sir E. Sabine's Magnetic Office collection, and are chiefly excerpts from foreign publications and reports.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund have been duly kept in order.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant.
H. McLaughlin, Librarian.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster ,, ,,
T. Gunter ,, ,,
W. J. Boxall, and seven other Assistants.

(Signed) Francis Galton,
Chairman of the Kew Committee.

November 29*th*, 1890.

The Kew Observatory. Account of Receipts and Payments for the year ending October 31st, 1890.

Dr. RECEIPTS.

	£	s.	d.
To Balance from 1888–89	562	5	11
Royal Society (Gassiot Trust)	487	10	0
Meteorological Office (Allowance)	400	0	0
Experimental Work	21	7	6
Verifications	1158	13	5
Rating of Watches and Chronometers	438	10	8
Commissions for Colonial and Foreign Institutions	351	5	6
Miscellaneous—'Times' Diagrams and Standard Thermometers	45	0	0
Meteorological Office for Postages and Porterages	2	15	7
	£3467	8	7

PAYMENTS. *Cr.*

	£	s.	d.	£	s.	d.
By Magnetic Observations, including estimated proportion of Working Expenses				190	3	6
Meteorological Observations, including estimated proportion of Working Expenses				298	0	5
Solar Observations, including estimated proportion of Working Expenses				74	16	11
Experimental work, including estimated proportion of Working Expenses				144	16	0
Verifications, including estimated proportion of Working Expenses				1042	16	4
Rating of Watches &c. " " "				355	9	8
Commissions for Colonial and Foreign Institutions				307	3	6
Alterations, Repairs, &c., of Building				50	4	0
Miscellaneous:—						
General Maintenance, Library, Office Work; Care, Lighting, and Heating				319	7	10
Exhibition, 'Times' Diagrams, &c., Pendulum				21	10	8
Meteorological Office for Postages and Porterages				3	16	11
Balances—Bank of England	574	12	8			
London and County Bank	71	13	9			
Cash in hand	12	16	5			
				659	2	10
				£3467	8	7

November 15, 1890. Examined and compared with Balance Sheet, and found correct. (Signed) ETTRICK W. CREAK, *Auditor.*

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
Balance as per Statement				659	2	10
Payments:—						
Meteorological Office Allowance, Experimental, &c.	91	16	7			
Verification and Rating Fees	187	1	4			
Commissions	46	12	0			
				325	9	11
Stock:—						
Photographic Paper	2	10	0			
Blank Magnetic Forms and Certificates	43	16	0			
Standard Thermometers	90	10	6			
				136	16	6
				£1121	9	3

ESTIMATED LIABILITIES.

	£	s.	d.
To Gas, Instruments, Experimental, and Contingencies	19	16	8
Rating and Verifications	3	12	0
Pendulum Account—Unspent Balance	120	14	4
Commissions	31	14	0
General Balance	945	12	3
	£1121	9	3

November 15, 1890. (Signed) G. M. WHIPPLE, *Superintendent.*

APPENDIX I.—Table I.

RESULTS OF WATCH TRIALS. Performance of the 27 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate. + Gaining. — Losing.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference of mean daily rate: Between pendant up and dial up.	Difference of mean daily rate: Between pendant up and pendant right.	Difference of mean daily rate: Between pendant up and pendant left.	Difference of mean daily rate: Between dial up and dial down.	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate.	Marks awarded for: Change of rate with change of position.	Marks awarded for: Temperature compensation.	Total Marks. 0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
A. E. Fridlander, Coventry	52669	Single overcoil, d.r.,* g.b.	+1·8	0·4	0·04	−1·6	+0·2	−1·8	+1·7	6·0	32·5	36·4	17·3	86·2
M. Klean & Co., London	62214	Double overcoil, s.r.,* fuzee, resilient	+0·8	0·4	0·09	+0·4	−0·1	−0·6	0·0	5·5	32·8	38·8	14·3	85·9
A. E. Fridlander, Coventry	52648	Single overcoil, d.r., g.b.	−0·1	0·4	0·02	+2·2	−0·7	−0·1	+0·8	4·5	32·3	34·5	18·6	85·4
G. Carley and Co., London	47219	Double overcoil, d.r., fusee	+1·0	0·5	0·02	+2·5	+0·1	+0·8	−0·3	5·75	30·3	35·8	18·9	85·0
Jos. White & Son, Coventry	32247	Double overcoil, s.r., g.b.	+1·4	0·3	0·06	+0·7	+1·2	+1·4	+4·1	7·25	33·7	34·6	16·2	84·6
Rotherham & Sons, Coventry	60538	Single overcoil, s.r., g.b.	+1·2	0·4	0·06	+0·2	+1·5	+2·5	+0·5	5·75	32·0	36·8	15·8	84·6
Jos. White & Son, Coventry	31817	Single overcoil, d.r., g.b.	+0·4	0·4	0·07	+1·8	−0·7	−0·4	−1·5	4·0	31·8	37·2	15·5	84·5
Baume & Co., London	108018	Single overcoil, g.b., tourbillon chronometer	−0·4	0·5	0·04	+0·2	+0·3	+0·3	+2·9	5·25	30·0	36·2	17·7	83·9
Stauffer, Son, & Co., London	120103	Single overcoil, d.r., g.b., bar-lever	+0·1	0·6	0·01	+4·0	−1·0	+0·7	−1·5	6·75	30·1	33·6	19·1	82·9
Nicole, Nielsen, & Co., London	10115	Duo-in-uno, d.r., g.b.	−0·2	0·4	0·08	+2·1	+0·9	−1·4	−1·3	6·25	31·8	36·2	14·7	82·7
A. E. Fridlander, Coventry	52656	Single overcoil, d.r., g.b.	+0·2	0·5	0·05	−1·1	−2·3	−3·5	+0·5	5·25	30·1	35·5	16·7	82·3
Usher & Cole, London	24397	Single overcoil, s.r., g.b.	+3·3	0·4	0·06	−2·4	−0·8	−4·5	−1·0	7·5	32·3	34·2	15·7	82·2
A. E. Fridlander, Coventry	52602	Single overcoil, d.r., fusee	+0·1	0·4	0·04	+0·9	+0·1	−2·8	−4·6	6·0	31·6	33·0	17·5	82·1
Stauffer, Son, & Co., London	126674	Single overcoil, d.r., g.b., bar-lever	+0·7	0·5	0·06	+0·1	−2·5	−0·5	+1·9	7·5	29·4	35·8	16·7	81·9
Usher & Cole, London	26805	Single overcoil, s.r., g.b.	+2·2	0·6	0·06	−0·1	−2·0	−2·0	−1·3	6·0	28·5	36·6	16·7	81·9
Usher & Cole, London	26659	Single overcoil, s.r., g.b.	+0·4	0·5	0·04	+1·7	−1·7	+3·4	−0·3	7·25	30·2	34·2	17·3	81·7
Rotherham & Sons, Coventry	86528	Single overcoil, s.r., g.b.	−0·6	0·6	0·06	+0·1	+0·4	−0·8	−1·0	7·25	27·9	38·0	15·8	81·7
Jos. Player, Coventry	25860	Single overcoil (palladium), d.r., g.b.	+2·0	0·5	0·04	−3·8	−3·3	−5·8	+0·6	10·5	29·3	34·8	17·4	81·5
A. Vuille, Chaux de-Fonds	10642	Single overcoil, d.r., g.b.	+3·3	0·6	0·01	−1·4	+3·0	+1·1	+1·2	7·0	27·2	35·0	19·3	81·5
Usher & Cole, London	26679	Single overcoil, s.r., fusee	+0·6	0·4	0·08	−1·0	+0·9	+2·0	−1·1	7·25	31·4	35·3	14·7	81·4
H. Golay, London	1929	Double overcoil, d.r., g.b.	+1·4	0·5	0·07	−3·5	−2·6	−4·6	+0·6	9·0	30·5	35·4	15·3	81·2
S. Yeomans, Coventry	52907	Single overcoil, s.r., g.b.	+0·9	0·6	0·03	+3·3	+5·7	+3·1	−1·0	8·0	28·6	34·5	18·1	81·2
H. Golay, London	1998	Double overcoil (palladium), s.r., g.b.	+2·9	0·5	0·05	+2·8	+1·4	+5·1	−2·7	9·0	30·9	33·3	16·9	81·1
Jos. Player, Coventry	24962	Single overcoil (palladium), d.r., g.b.	−0·6	0·5	0·06	+3·1	+2·0	+1·1	−0·2	8·25	29·4	36·0	15·7	81·1
Rotherham & Sons, Coventry	84303	Single overcoil, s.r., g.b.	−2·4	0·5	0·06	−0·1	−3·3	−2·3	−1·6	6·5	29·1	35·4	16·6	81·1
B. Bonniksen, Coventry	1441	Single overcoil, s.r., g.b.	−0·6	0·5	0·02	+3·2	+0·1	−0·5	+0·8	7·5	30·1	32·9	18·0	81·0
C. J. Hill, Coventry	138154	Single overcoil, s.r., g.b.	−1·8	0·6	0·07	+1·1	−0·1	+0·8	+0·4	6·25	28·2	37·7	15·1	81·0

* d.r., double-roller; s.r., single-roller; g.b., going barrel.

APPENDIX I.—Table II.

Highest Records obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for			Total marks, 0—100.
			Variation.	Position.	Temperature.	
Minute and seconds chronograph, minute repeater, and perpetual calendar, with phases of the moon	2806	H. Golay, London	27·6	26·5	14·9	69·0
" " " "	2285*	S. Smith and Son, London	24·9	28·6	12·5	66·0
Minute and seconds chronograph and repeater	2839*	Baume and Co., London	29·5	34·3	14·7	78·5
" " " "	2994	H. Golay, "	29·2	33·6	15·3	78·1
" " " "	2460	S. Smith and Son, "	21·7	37·7	17·7	77·1
Minute and split seconds chronograph	10115	Nicole, Nielsen, & Co., London	31·8	36·2	14·7	82·7
" " "	86965	Rotherham and Sons, Coventry	25·7	33·0	18·5	77·2
" " "	14770	S. Smith and Son, London	25·6	34·2	15·0	74·8
Minute and seconds chronograph	80538	Rotherham and Sons, Coventry	32·0	36·8	15·8	84·6
" " "	1929	H. Golay, London	30·5	35·4	15·3	81·2
" " "	$\frac{1}{4011}$	J. Hammersley, London	25·0	37·0	17·6	79·6
" " "	90405	Rotherham and Sons, Coventry	32 2	29·5	16·3	78·0
Ordinary seconds chronograph and repeater	3383	H. Golay, London	25·7	31·0	17·0	73·7
" " " "	36695	" "	27·7	28·9	15·2	71·8
" " " "	30225	S. Smith and Son, London	19·9	33·9	17·2	71·0
Repeater	52656	A. E. Fridlander, Coventry	30·1	35·5	16·7	82·3
"	36591	H. Golay, London	26·6	32·2	18·5	77·3
"	3361	" "	27·1	31·5	17·6	76·2
"	36692	" "	25·8	34·4	15·9	76·1

* Split seconds.

APPENDIX II.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete. Pair 9-inch Dip-Needles with Bar Magnets	1883 1887
Professor O. J. Lodge, F.R.S.	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Prof. T. E. Thorpe, F.R.S.	Tripod Stand	1886
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885

"On the Relation between the Magnetic Permeability of Rocks and Regional Magnetic Disturbances." By A. W. RÜCKER, M.A., F.R.S. Received May 30,—Read June 19, 1890.

The investigation, of which an account is given in this paper, was undertaken with the object of throwing light on the causes of local magnetic disturbances. The two main theories which have hitherto been proposed attribute local perturbations of the needle to earth currents and to magnetic rocks respectively.

In the Bakerian Lecture for 1889 ('Phil. Trans.,' A. 1890, p. 53), Dr. Thorpe and I compared the directions of the disturbing magnetic forces found by us to exist near Melton Mowbray and Reading with the results of a survey of the local earth currents made in the neighbourhood of those places under the direction of Mr. Preece, F.R.S. No connexion could be traced between either the intensities or the directions of the currents and the magnetic forces, and the result of the investigation was thus opposed to the view that they are cause and effect. As far as I am aware, however, no attempt has hitherto been made to determine whether the mere presence in the earth's magnetic field of such iron-bearing rocks as actually exist, and which must certainly produce magnetic disturbances, suffices to account for such disturbances as are actually observed. This enquiry is obviously complementary to the comparison of the disturbing forces with the earth currents in the same neighbourhood, but the necessary data have only lately been accumulated. The recent magnetic survey has for the first time placed at our disposal facts as to the magnitude of the disturbing magnetic forces in the United Kingdom, and the measurements described below give us some idea of the order of the magnitude of the permeabilities of magnetic rocks. The present investigation is thus divided into two parts, viz.:—

(1.) A determination of the magnetic susceptibility of a number of rock specimens.

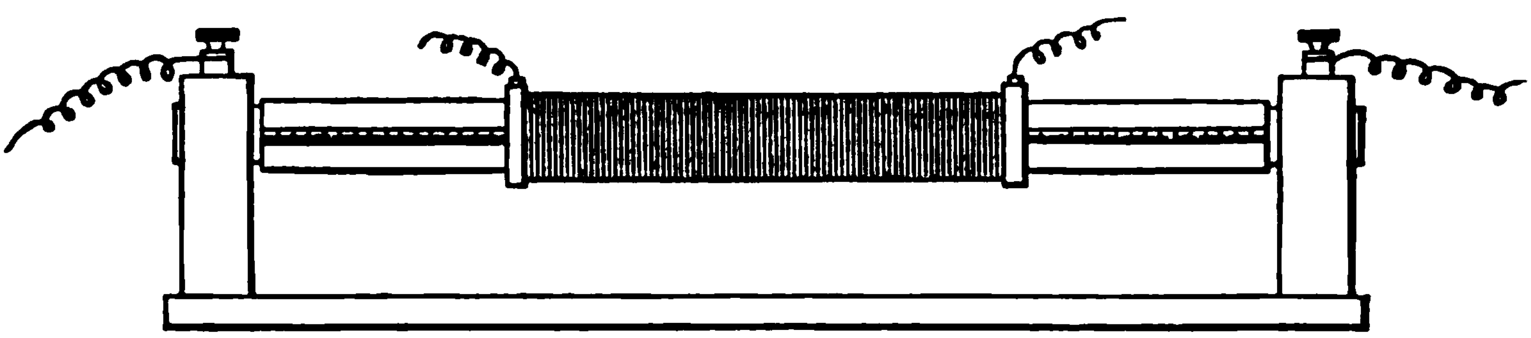
(2.) An enquiry as to the order of the magnitude of the magnetic disturbances which the presence of such rocks in the earth's magnetic field would produce.

The first part has been carried out by Mr. Highfield, Assoc. N.S.S., and Mr. Jarratt, Scholar Elect of Trin. Coll., Cambridge, both of whom are students in the Physical Laboratory of the Normal School of Science and Royal School of Mines. I am under a great obligation to these gentlemen for their share in the work. They constructed all the special apparatus required, and have made all the measurements recorded in this paper, and I am indebted to them not only for care and skill, but also for several very useful suggestions.

In selecting a method, it was important to be able to deal with small fragments of rock, and also to avoid the necessity of having to shape them into definite forms. The magnetic properties of a district can only be ascertained by the examination of a large number of specimens, and this would be practically impossible unless the above conditions were fulfilled. Great accuracy, though desirable, is not equally important. Different specimens of the same rock differ so widely in their magnetic qualities that a comparatively rough measurement is sufficient. It will, however, be seen in the sequel that the observations made are in satisfactory agreement.

To meet these requirements the following scheme was devised. A series of standard magnetic fluids were made by suspending magnetic oxide of iron in various proportions of glycerine. The susceptibilities of these mixtures were determined absolutely by the apparatus described below, and specimens of the rocks were compared with them by means of Professor Hughes' induction balance. For this purpose, equal volumes of a mixture were placed in two similar test-tubes, which were inserted in the cups of the balance, and silence was obtained by means of a compensator. The rock to be tested was

FIG. 1.



now immersed in one of the mixtures, and an equal volume of liquid having been abstracted, the zero was redetermined. Two mixtures were thus found, to the susceptibilities of which that of the rock under experiment was intermediate. The compensator used, though identical in principle, differed in form from that employed by Professor Hughes. The primary current passed through two solenoids wound in opposite directions about the two ends of a tube. Over these another larger tube could be moved in either direction, and round it another solenoid was wound which formed part of the secondary circuit. The position of this secondary solenoid was read off on a millimetre scale attached to the exterior of the inner tube. The two primary coils tended to produce induced currents in opposite directions, and thus, by moving the secondary coil in one direction or the other, silence could be obtained.

If x_1 and x_2 are the distances (measured in opposite directions) through which the compensator had to be moved to produce silence when the specimen was introduced into the first and second liquid

respectively, and if k, k_1, and k_2 are the susceptibilities of the rock and the two liquids, k may be calculated from the formula

$$\frac{x_1}{x_2} = \frac{k-k_1}{k_2-k},$$

or

$$k = k_1 + \frac{x_1}{x_1+x_2} \times (k_2-k_1).$$

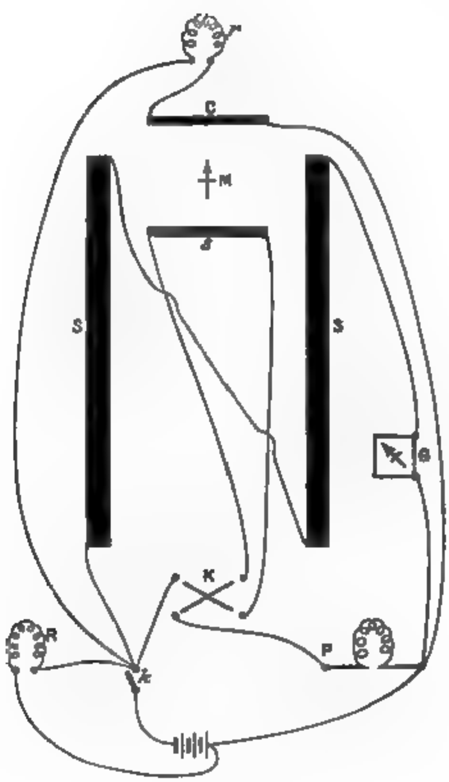
My thanks are due to Professor Judd, F.R.S., for the large number of rock specimens which he has placed at my disposal. Nearly all which have been used have been provided by him. This aid has been especially important from the fact that the attention which he has paid to the geology of the West of Scotland has made his collections rich in basaltic rocks gathered from that district, which is within the area of the recent magnetic survey, and is remarkable for the magnitude, not only of the local, but also of the regional, magnetic disturbances of which it is the seat. He has also been good enough to have sections made of a number of the rocks examined by the induction balance. Some of the results thus obtained are referred to below, but we hope to extend this part of the enquiry in the immediate future.

PART I.—*On the Magnetic Susceptibility of Rocks.*

The apparatus used for determining the absolute susceptibilities of the mixtures is shown in fig. 2.

Primarily it consisted of a magnet attached to a mirror which was delicately suspended in a glass case by means of a quartz fibre. Two large solenoids wound upon glass tubes were placed at equal distances east and west of the needle. A smaller tube, which could be filled with any of the mixtures, was arranged so as to slide into and out of either solenoid. The deflections when the tube was inserted first into one solenoid and then into the other were noted, and from these the susceptibility of the mixture could be calculated when the strength of the field in which the magnet was suspended was known. The necessity for the determination of this datum was obviated by also deflecting the magnet by a small solenoid, the moment of which was calculated from the number of coils, the length, and the strength of the current flowing through it. In the figure, M represents the magnetometer, and SS the two large solenoids into which the tube containing the liquid is capable of sliding. The ends of the solenoids projected for some 7 or 8 cm. beyond the ends of the tube, which was always brought up to the same position within the solenoids by means of fiducial marks. The sensitiveness of the magnetometer was adjusted by a control magnet placed above it, acting so as to reduce the earth's

FIG. 2.



field. In this way an oscillation-period of 25 seconds could be obtained, although it was found in practice to be unnecessary to increase it beyond about 10 seconds. At k there is a key which divides the current, part of which goes through the two large solenoids SS, flowing in the same direction in each, and thence through the galvanometer G back to the battery. Another part goes through the reversing key K to the small deflecting solenoid s and thence through the Post Office bridge P back to the battery. The resistance used in the Post Office bridge was about 200 ohms, while that of the main circuit was about 10 ohms, so that the amount of current shunted was comparatively small; nevertheless, to prevent any error being thus caused, another shunt circuit, of resistance R, equal to that in the Post Office bridge, was introduced, through which the current passed when the small solenoid was not in use.

As the effects produced by the two principal solenoids on the magnetometer were not exactly balanced when they were placed at equal

distances from it, it was found necessary to introduce another small compensating solenoid (C) which could be so adjusted as to neutralise the residual effect.

The mirror was raised sufficiently above the horizontal plane through the axes of the two solenoids to permit a beam of light being thrown upon it and reflected to a scale. An experiment was performed as follows:—

The small solenoid circuit *s* was first thrown out of connexion, and the main circuit, together with the shunt R, put in. The zero reading, as given by the magnetometer, having been taken, the apparatus was so arranged that the zero did not alter when the current was put on, or off, or reversed. The tube was now inserted into each of the solenoids in turn, the deflections were noted, and afterwards the zero was again taken to show that no change had occurred during the experiment. The shunt R was then thrown out, and the circuit containing the small solenoid *s* put in, the resistance in the Post Office bridge having been previously arranged so as to give a deflection of much the same magnitude as that due to the introduction of the tube containing the mixture of magnetic oxide and glycerine. Knowing the deflection which is given by a solenoid of known moment, and that produced by the introduction of a definite amount of mixture under the same conditions as regards the sensibility of the magnetometer, we are able to calculate the susceptibility of the mixture, as follows:—

Let $2b$ be the length of the tube.

ξ, η, ζ the co-ordinates of the centre of the magnet referred to three rectangular axes, passing through the middle point of the axis of the tube, parallel to its length, and perpendicular to its length in the horizontal and vertical directions respectively.

σ, σ_1, the distances from the centre of the magnet of the feet of the perpendiculars let fall from the ends of the tube on the horizontal plane through the magnet.

It is then easy to show that if the length of the magnet is small as compared with σ and σ', and if p be the strength of the pole induced at the end of the tube,

$$p = \frac{\mathrm{F}\,.\,d}{(\mathrm{M}d + 2\mathrm{N}\,.\,\eta\,.\,\mathrm{D})},$$

where F is the strength of the field in which the magnet is placed,

d the deflection produced by the introduction of the tube containing the liquid into the solenoid,

D the distance of the scale from the magnet,

$$M = \frac{\xi - b}{(\sigma^2+\xi^2)^{\frac{3}{2}}} - \frac{\xi+b}{(\sigma_1^2+\xi^2)^{\frac{3}{2}}}$$

and $$N = \frac{1}{(\sigma^2+\xi^2)^{\frac{3}{2}}} - \frac{1}{(\sigma_1^2+\xi_1^2)^{\frac{3}{2}}}.$$

With the distances actually adopted in one experiment;

$$M = -0\cdot000273, \quad d < 10\text{cm.},$$

$$N = 0\cdot000170, \quad \eta = 17\cdot0 \text{ cm.}, \quad D = 103 \text{ cm.}$$

Hence, the term $M \,.\, d$ is small, and may be neglected, and, therefore,

$$p = \frac{Fd}{2 \,.\, N \,.\, \eta \,.\, D}.$$

If I be the intensity of magnetisation of the liquid,
A the area of the tube,
n the number of turns per cm. in the solenoid,
C the strength of the current in absolute units,
κ the susceptibility of the liquid,

$$\kappa = \frac{I}{4 \,.\, \pi \,.\, n \,.\, C} = \frac{F \,.\, d}{8 \,.\, \pi \,.\, n \,.\, C \,.\, N \,.\, \eta \,.\, D \,.\, A},$$

where $8 \,.\, \pi \,.\, n \,.\, N \,.\, \eta \,.\, D \,.\, A$ is constant for any one position of the apparatus, and $= N_1$ say;

therefore $$\kappa = \frac{F \,.\, d}{N_1 \,.\, C}.$$

The solenoids were practically identical in construction, and if $d_1 - d_2$ be the algebraical difference of the deflections right and left when the tube is introduced into the two solenoids,

$$\kappa = \frac{F(d_1 - d_2)}{2 \,.\, N_1 \,.\, C}.$$

To determine F the auxiliary solenoid was used. It was placed perpendicular to the axis of the magnet, which bisected it. Its length being $2a$, number of turns per cm. n', area A', the distance of its poles from the centre of the magnet u, and the deflections being d'_1 and d'_2 to the left and to the right respectively, when the current C' circulated through it,

$$F = \frac{8 \,.\, a \,.\, n' \,.\, C' \,.\, A' \,.\, D}{u^3(d'_1 - d'_2)}.$$

Hence, $$\kappa = \frac{1}{2\pi} \cdot \frac{(d_1 - d_2)}{(d'_1 - d'_2)} \cdot \frac{C'}{C} \cdot \frac{n'}{n} \cdot \frac{A'}{A} \cdot \frac{a}{\eta} \cdot \frac{1}{u^3} \cdot \frac{1}{N},$$

or, since the currents varied inversely as the resistances of the principal and shunt circuits,

$$\kappa = B \frac{d_1 - d_2}{d'_1 - d'_2}$$

where B is a constant depending on the dimensions and resistances of the various parts of the apparatus.

In one series of experiments, chosen haphazard for illustration, the values of the different quantities were as follows :—

$$\sigma = 17{\cdot}16 \text{ cm.}, \qquad \sigma_1 = 40{\cdot}31 \text{ cm.}, \qquad \zeta = 3{\cdot}7 \text{ cm.};$$

therefore $N = 0{\cdot}0001696$.

The resistance of the principal circuit $= 10{\cdot}9$ ohms,

" " shunt " $= 141$ "

$$n' = 30{\cdot}81, \qquad n = 11{\cdot}25,$$

$$A' = 1{\cdot}5836 \text{ sq. cm.}, \qquad A = 6{\cdot}7424 \text{ sq. cm.},$$

$$a = 13{\cdot}0 \text{ cm.}, \qquad \eta = 17{\cdot}1 \text{ cm.},$$

$$u = 22{\cdot}61 \text{ cm.},$$

$$(d_1 - d_2) = 4{\cdot}3 \text{ cm.}, \qquad (d'_1 - d'_2) = 8{\cdot}5 \text{ cm.};$$

therefore $\kappa = 0{\cdot}00158$.

In each experiment a deflection was the mean of two readings taken with the current direct, and reversed when the tube was in each solenoid, and with the current direct and reversed in the case of the auxiliary solenoid. The effect of the earth's magnetic field was thus eliminated.

Various possible sources of error had to be investigated. In the first place, the suspended magnetic oxide might gradually settle in the tubes, or, under the influence of the current, the particles might tend to set themselves with their axes parallel to the axis of the solenoid, as in Sir William Grove's well-known lecture experiment. These effects were most to be feared in the case of the strong mixtures. It was found that if the magnetic oxide were dried it caked and it was impossible afterwards to suspend it in a state of fine and equal division in the glycerine. Hence the mixtures were made by mingling known volumes of one standard mixture of magnetic oxide and water with glycerine. Thus the stronger mixtures were the more aqueous, and therefore the less viscous. To investigate the possible error due to settling, the following experiments were made :—

The mixtures were allowed to remain in the tube for 30 minutes: no difference in the deflection could be detected in the case of the weaker mixtures; but for the strongest we obtained the following results. The deflections are throughout measured in cm.

Zero		0·00
Deflection with tube in		7·31
" " "	after 30 mins..	7·00

Zero		0·00
Deflection with tube in		8·38
" " "	after 30 mins..	8·11

During the first few minutes no appreciable falling off was observed, so that, as the time required for an experiment does not exceed a minute or two, any error arising from this cause may be neglected. Care was also taken to rotate the tubes frequently, and to empty them and pour the liquid in again at short intervals. In order to test the accuracy of the whole arrangement, several series of experiments were made with the apparatus set up in different positions, and the values obtained were such as to show that no serious discrepancy existed. In the following table the fractions in the first column are the ratios of the volumes of the standard mixture of water and magnetic oxide to that of the glycerine, and they may therefore be called the strengths of the mixtures. In the other columns are the susceptibilities obtained in each case with a completely different arrangement of the apparatus.

s = Strength.	κ.	κ.	κ.
$\frac{1}{5}$	0·00126	0·00115	0·00125
$\frac{2}{9}$	..	144	137
$\frac{1}{4}$	158	153	155
$\frac{2}{7}$	..	174	168
$\frac{1}{3}$	218	214	209

Six weeks after the last of the preceding series had been taken. another set of experiments was made in order to test the invariability of the magnetic properties of the mixtures. No change whatever could be detected, as will be seen from the following table of results:—

Strength = s.	κ.
$\frac{1}{5}$	0·00120
$\frac{2}{9}$	137
$\frac{1}{4}$	159
$\frac{2}{7}$	163
$\frac{1}{3}$	208

If now we take the means of the results of these four series of experiments as giving the values of the susceptibilities, and divide each number by the strength of the mixture, the ratio is found to be nearly constant. It must be remembered that errors which cause deviations from the mean value are in part, and probably in large part, due to uncertainty as to the exact composition of each mixture. As the magnetic oxide settles quickly in water, the amounts added to the glycerine were probably only approximately proportional to the volumes used; but, as the susceptibility of each mixture is absolutely determined without reference to its supposed composition, this will not affect the accuracy of the results.

Strength = s.	Mean value of κ.	$\frac{\kappa}{s}$.
$\frac{1}{5}$	0·001215	0·00607
$\frac{2}{9}$	1393	627
$\frac{1}{4}$	1562	625
$\frac{2}{7}$	1683	589
$\frac{1}{3}$	2122	637

The numbers in the last column are consistent with the view that the susceptibility of any mixture varies directly as the percentage of magnetic oxide which it contains. The matter may be further tested by means of the sets of experiments described immediately below, in which the susceptibilities of three of the liquids (including the weakest and the strongest mixtures) were again measured. The mean results are as follows:—

Strength = s.	Mean value of κ.	$\frac{\kappa}{s}$.
$\frac{1}{5}$	0·00124	0·00620
$\frac{1}{4}$	159	636
$\frac{1}{3}$	204	612

In this case the strongest mixture gives the smallest result. On the whole, then, and for the purposes of this investigation, the values of κ/s must be considered as independent of the strength of the mixture. Any difference which exists could only be certainly detected by a very careful determination of the quantity of magnetic oxide present in the unit of volume in each case. Since this law holds good within the limits of the series of liquids the suscepti-

bilities of which could be accurately determined, it may be safely inferred that for weaker mixtures, at any rate, it will still be true, and so a number of mixtures weaker than one-fifth were prepared, and their susceptibilities calculated by proportion from those of the stronger ones. Values so obtained will, however, be subject to the error of mixing.

Finally, a series of experiments, referred to above, was made to determine whether the permeabilities of the mixtures varied with the magnetic force. The susceptibilities obtained when 2, 3, and 6 Grove's cells were used in turn are given in the following table:—

Strength.	Six cells.	Three cells.	Two cells.
$\frac{1}{5}$	0·00122	0·00127	0·00123
$\frac{1}{4}$	164	162	150
$\frac{1}{3}$	209	199	203

The sums of the significant figures in the three columns are 495, 488, and 476 respectively, thus indicating a slight increase of permeability with the magnetic force. The differences between the individual observations are, however, too great to allow us to rely on this result, and the table can only be considered as proving that no serious error will occur if we assume that the permeabilities of the mixtures are independent of the magnetic force.

[Added Sept. 12, 1890.—Experiments made afterwards confirmed this view and extended the range over which its accuracy was tested. The weakest field employed was about twice the earth's field in the United Kingdom.]

It would have been difficult to obtain accurate results with a weaker field, but as the law of proportionality between the magnetic force and the induction appears to hold very approximately for forces between 5·0 and 1·7 C.G.S. units, it is probable that it is also valid for smaller values. It is true that Silow ('Wiedemann's Annalen,' vol. 11, 1880, p. 330) has stated that the susceptibility of ferric chloride is a maximum when the inducing force is about 0·4 C.G.S. unit, and that for that value it is between two and three times as great as for fields at strengths such as those at which we have experimented, but the change in a range of magnetic force much less than 1·0 to 5·0 was very marked. Thus, between forces of about 1·4 and 2·5, the susceptibility altered by about 13 per cent. An effect such as this could not possibly have escaped our notice, and there can be no doubt that for forces such as those with which we have dealt the susceptibility of magnetite changes

very slowly, and that the variation is not sufficient to affect seriously the argument of this paper.

We now turn to the method of comparing the susceptibilities of the rock specimens with those of the liquid by means of Professor Hughes' induction balance.

In the first place it was necessary to make certain that the effects observed were due only to the permeabilities, and not to the conductivities, of the bodies under investigation. The weaker liquids were practically non-conductors, but the stronger ones conducted feebly. When, however, a solution of salt in water, of rather greater conductivity than the strongest mixture, was introduced into the balance, which had previously been adjusted, no sound whatever could be detected, thereby proving that the very different effects obtained with the magnetic oxide were not, in any way, due to Foucault currents in the mixture. Two of the rocks which produced the greatest effect in the balance were also chipped out into the form of horse-shoes, and by dipping the ends into two mercury cups or into two cups containing acid and water, they were used to complete circuits, in which a mirror galvanometer was included. They appeared, as thus tested, to be non-conductors. We are, therefore, confident that the experiments are not vitiated by Foucault currents set up within either the liquids or the rocks.

The first test applied to the method was to measure by the aid of the balance the susceptibilities of the different mixtures relatively to each other. Thus in the case of three liquids *a*, *b*, and *c*, say, the susceptibility of *b* was found by using the values of the susceptibilities of *a* and *c* which had been obtained by the absolute method. In the following table the numbers thus obtained are compared with those given directly by the absolute method :—

s.	κ measured by—	
	Magnetometer method.	Induction balance.
$\frac{3}{5}$	0·00139	0·00135
$\frac{2}{7}$	168	172
$\frac{1}{3}$	212	208

The agreement between the last two columns is sufficient to justify the induction balance method. The strengths of the fields in the balance were different from those employed in the absolute method, and Foucault currents might affect the results. It is clear, however, that neither of these possible causes of difference produces any

appreciable error. Even if the magnetic force and the induction are not strictly proportional, the ratio of the permeabilities of the liquids is, to the degree of accuracy attained, the same in the balance as in the solenoid.

Finally, the method was applied to a specimen of basalt which Professors Thorpe and Rücker had brought from the Island of Canna, in the West of Scotland. A piece of this had been cut into the form of a rectangular bar, and its magnetic properties had been investigated by Dr. Hoffert. His experiments are described in the published account of the magnetic survey ('Phil. Trans.,' A, 1890). The permanent magnetisation was determined by three methods, and the susceptibility was found from the times of vibration when the bar was suspended in a known magnetic field with the directions of permanent and induced magnetisation alternately coincident and opposed. Dr. Hoffert found that the value of κ was about 0·0015. Unfortunately, the particular bar used by him has been mislaid, but we have measured the permeability of another fragment of the same specimen, and find—

$$\kappa = 0{\cdot}00132.$$

Observations to be described below prove that differences such as this exist between specimens of the same rock. We do not therefore regard these numbers as giving any test of the accuracy either of Dr. Hoffert's or our own observations, but the agreement between them is sufficient to prove that there can be no doubt as to the order of the magnitudes of the quantities under discussion.

The range over which the instrument could be employed was between susceptibilities about five times greater and ten less than that of Canna basalt, *i.e.*, from about 0·00792 to 0·00013. The higher of these values could only be obtained by extrapolation, as liquids of such great permeability could only be formed by using so small a quantity of glycerine that the magnetic oxide settled too quickly to enable us to obtain reliable results. In the tables given below, the statement that the magnetic susceptibility of a substance is zero means only that it is distinctly less than that of the weakest liquid employed, *i.e.*, than about 0·00013. The details of a single experiment are given by way of example. The readings of the compensator scale are in centimetres.

Experiment.

Equal volumes of the liquid of strength 2/9 were placed in test-tubes in the two arms of the balance.

$$\text{Zero readings}\begin{cases}10{\cdot}1\\10{\cdot}2\\10{\cdot}0\\10{\cdot}15\end{cases}\qquad \text{Mean} = 10{\cdot}11 = z_1.$$

A specimen of Canna basalt was placed in the left-hand test-tube, and liquid abstracted until the volume was the same as before.

$$\text{Readings} \ldots \begin{cases} 9{\cdot}8 \\ 9{\cdot}9 \\ 9{\cdot}8 \end{cases} \qquad \text{Mean} = 9{\cdot}83 = r_1 ;$$

therefore $$x_1 = (z_1 - r_1) = 0{\cdot}28.$$

Equal volumes of the liquid of strength 1/5 were now taken.

$$\text{Zero readings} \begin{cases} 10{\cdot}15 \\ 9{\cdot}9 \\ 10{\cdot}0 \\ 10{\cdot}2 \end{cases} \qquad \text{Mean} = 10{\cdot}06 = z_2.$$

The same specimen of rock was now placed in the left-hand test-tube, after having been carefully washed and dried.

$$\text{Readings} \ldots \begin{cases} 10{\cdot}0 \\ 10{\cdot}2 \\ 10{\cdot}0 \\ 10{\cdot}1 \end{cases} \qquad \text{Mean} = 10{\cdot}08 = r_2.$$

$$x_2 = (z_2 - r_2) = 0{\cdot}02.$$

Hence, by the formula given above,

$$\kappa = 0{\cdot}001227.$$

On the whole then, we think that the various tests which have been applied to it prove that the method employed fulfils the required conditions very satisfactorily. It is not capable of giving results of the last degree of accuracy, but it enables us to measure quickly and certainly, with only a small percentage error, the permeabilities of rock specimens without the labour and expense involved in shaping them into definite geometrical forms.

The method, too, has the advantage that, when once the permeabilities of the standard liquids are determined, the apparatus can be used anywhere. If therefore it were desirable to institute a close comparison between the magnetic disturbances and the magnetic permeabilities of the rocks in a given district, and it were important that the investigator should become at once acquainted with his results, it would be quite practicable to transport the apparatus required to the scene of the investigation, and to determine the magnetic properties of the specimens in any convenient room within a few hours of their collection.

Our observations on rock specimens may be divided into three

groups, according as they were (α) non-magnetic, (β) magnetic but not basaltic, (γ) basaltic.

Of the first group, we tried a number of specimens, many of which were *a priori* certain not to be magnetic. Some of them, however, were just as likely to be conductors as the magnetic rocks; and the fact, therefore, that they have been tried, and produce no effect, strengthens the view that the measurement of the permeabilities was not affected by the conductivities of the specimens. Among those submitted to experiment were limestones, sandstones, mica- and hornblende-schists, granite with tourmaline, red granite, trachyte, felsite, rhyolite, gabbro, muscovite granite, luxullianite, various diorites, and hæmatite.

Two specimens of Archæan gneiss, brought by Dr. Thorpe from Loch Maddy, in the Outer Hebrides, were found to be practically non-magnetic.

We have also, through the kindness of Professor Judd, had the opportunity of testing the specimens of Silurian rocks and red sandstones obtained from the Palæozoic ridge by deep borings near London. Fragments from Kentish Town, Richmond, Meux's Brewery, and Ware were tried and found to be non-magnetic.

Turning next to specimens of other than basaltic rocks which were found to be magnetic, we obtained the following results:—

	κ.
Phonolite	0·00070
Dolerite	94
Trachyte	39
Melaphyre	39
Tourmaline granite .	24
Syenite	104

It will be noticed that several of these are rocks of the same kind as those of which other specimens were found to be non-magnetic. This is an example of the fact that the permeabilities of different portions of the same rock are very various, and that no conclusion can be drawn unless a large number of specimens have been examined.

Special attention having been given in the recent magnetic survey to the magnetic disturbances produced by the Malvern Hills, it was thought that a detailed investigation of their magnetic properties would be interesting. Mr. Highfield, therefore, paid a visit to Malvern for the purpose of collecting specimens. The position at which any specimen was found was marked on the spot on an Ordnance map carried for the purpose. It is, perhaps, hardly neces-

sary to reproduce this map here, but notes are appended to the table given below which indicate the point on the range from which the specimen was obtained. Care was taken that the specimens should not be weathered. It may be well to add that the Malvern Hills are a range of hornblendic rocks, bounded on the east by a great fault, which divides them from the red marls of the Valley of the Severn. On the western side the igneous rocks emerge from under Wenlock

No. of specimen.	Position of station.	Mean value of κ.
1*a*, *b*, *c*	Quarry at North Malvern, near the tank	0·00012, 0, 0
2*a*, *b*	On the crest at the extreme north end of the range	46, 25
3*a*, *b*, *c*	On the ridge between the peaks of the North Hill	0, 12, 59
4*a*, *b*	Near to the summit of the North Hill	0, 102
5*a*, *b*	Near St. Ann's Well	0, 0
6*a*, *b*	Near to the top of the Worcestershire Beacon	69, 90
7*a*, *b*, *c*	Near to the footpath, considerably below and a little to the south of the top of the Worcestershire Beacon	0, 0, 12
8*a*, *b*	On the ridge about ¼ mile south of the top of the Worcestershire Beacon	0, 0
9*a*, *b*	On the ridge above the railway tunnel	0, 0
10*a*, *b*	In a cutting through the ridge above Malvern Wells	113, 0
11*a*, *b*	On the ridge nearly due east of Brand Hall (Ordnance map)	30, 0
12*a*, *b*	On the ridge on the north side of the Ledbury Road	139, 0
13	On the Herefordshire Beacon, about a furlong south-east of the encampment	0
14	On the western side of the Herefordshire Beacon, about 300 yards south of 13	0
15	At the south end of the Herefordshire Beacon, above Hill Farm	0

limestone and Silurian rocks, which are bent upwards on their flanks. The collection of specimens was composed entirely of the crystalline rocks: it was begun at the extreme north end of the range, and continued as far south as the Herefordshire Beacon, a distance of about 5 miles. In most cases two specimens were taken at each spot, and in the table these are indicated by the same number followed by different letters.

As the susceptibilities of the rocks varied so considerably, Professor Judd was good enough to have sections made of some of those which differed most widely. The following report made by him shows a satisfactory agreement between the indications of the induction balance and microscopic examinations :—

Specimen.	κ.	Remarks.
12*a*	0·00139	Large amount of magnetite, well crystallised; much pyrites.
10*a*	113	Rather smaller quantity of magnetite, and in smaller crystals.
11*a*	30	Magnetite, small in quantity, and sporadically distributed.
7*c*	12	Magnetite, very small in quantity (possibly only titanoferrite present).
12*b*	zero	Magnetite, very small in quantity (possibly only titanoferrite present).

As was to be expected, basic rocks proved to be the most strongly magnetic; but it is well at once to emphasise the fact that powerful permanent magnetisation affords no proof of high permeability. Thus a specimen of rock from the Peak of the Island of Ascension, kindly supplied to us for examination by Captain Creak, F.R.S., strongly attracted and repelled the pole of a compass needle. Its susceptibility, however, was only moderately large, being 0·00122.

We have collected in the following table the results of our measurements on basic rocks :—

Specimen of—	Locality.	κ.	Mean value of κ.
Dolerite	Ratho, Edinburgh	0·00113	
Enstatite-andesite	Newport, Fife	59	
,, ,,	Durham	134	
Porphyritic basalt	Schemnitz, Hungary	109	
Basalt	Faroe Islands	116	
Olivine-diabase	Nabe, Rhine	47	
Basalt	Unkel-on-Rhine	45	
,,	Rowley Regis	118	
,,	Giant's Causeway	27	0·00024
,,	,, ,,	21	
Olivine-gabbro	Skye	697	0·00561
,, ,,	Cuilin Hill, Skye	747	
Fine-grained gabbro	Skye	246	
Olivine-gabbro	,,	553	
Gabbro	Deer Forest, Ardnamurchan	660	0·00420
,,	,, ,,	632	
,,	,, ,,	307	
,,	,, ,,	83	
Dolerite	Tobermorey, Mull	0·00147	0·00163
Porphyritic basalt	,, ,,	231	
Olivine ,,	,, ,,	74	
,, ,,	,, ,,	184	
Basalt	Tobermorey Harbour	209	
Porphyritic basalt	Fishguard ,,	61	
,, dolerite	Mull	155	
Platy basalt	,,	113	
Gabbro	Mhaim Clackaig, Mull	100	
,,	Ben More ,,	146	
Basalt	Dumfrin ,,	114	
Olivine gabbro	Ben More ,,	429	
Dolerite	Dun-da-gaioth ,,	156	
Basalt	Staffa	0·00048	0·00062
,,	,,	77	
Gabbro	Loch Coruiskh, Skye	0·00049	0·00237
,,	,,	164	
,,	,,	628	
,,	,,	27	
,,	,,	362	
,,	,,	82	
,,	,,	153	
,,	,,	284	
,,	,,	75	
,,	,,	684	
,,	,,	99	

These results are sufficiently numerous to justify their employment in the calculations which follow. They prove that, in spite of great variations between individual specimens, the average susceptibility of

basic rocks is relatively very high. The average of all the specimens from the west of Scotland and from Ireland is 0·00245. If we exclude the Giant's Causeway and Staffa, it is 0·00271, which is thus the average of all the specimens tested from a district nearly 70 miles in length. There would, therefore, be nothing absurd in the supposition that equally large values obtained over equally large areas elsewhere; but in the calculations the assumed susceptibility is the much smaller value given by the Mull specimens, viz., 0·0016.

An experiment was made on the effects of temperature on the permeability of magnetite. It was only of a rough preliminary kind, but the result was quite clear, and further and more elaborate experiments on the same point are about to be undertaken in the laboratory at South Kensington.

The interior of one of the cups of the Hughes' induction balance was lined with asbestos cloth, and a fragment of non-magnetic granite which had been heated to incipient redness in the flame of a Bunsen burner was introduced into it. The balance which had been previously obtained was quite undisturbed. The same experiment was then repeated with a piece of magnetite. The introduction of the rock at once caused the telephone to "speak," but silence was quickly obtained by turning the screw by which the parallelism of the primary and secondary coils is secured. The compensator used in the previous experiments had not sufficient range, and readings were taken by a paper scale of degrees attached to the screw head. As the magnetite cooled the zero altered, and, in order to maintain silence, it was necessary to keep turning the screw in the direction which indicated that the permeability of the specimen was decreasing. The stone was allowed to cool for half an hour, and the total alteration of the zero measured. It was then removed altogether, and the new position of silence found. In one experiment the following values were obtained. The figures in the second column indicate the number of degrees through which the screw was turned from the first zero obtained after the introduction of the hot magnetite:—

	Reading.
Magnetite, hot.....	0°
,, cold....	320
,, removed	790

This result proves that, as in the case of iron, the permeability of magnetite increases as the temperature rises, the increase in the experiment just described being about 70 per cent. A second experi-

ment gave about 60 per cent. It is, of course, probable that if the temperature is raised sufficiently the permeability of magnetite, like that of iron, will rapidly diminish, and that, after a certain temperature is reached, it will cease to be magnetic. This point also we hope to investigate further.

PART II.—*On Regional Magnetic Disturbances.*

In attempting to base calculations upon the permeabilities measured by Messrs. Highfield and Jarratt, it is necessary to make some assumptions as to the magnetic state of the earth's crust.

The average increase of temperature with depth is about 1° C. for every 90 feet, and if this rate obtains for a depth of several thousand feet, the temperature would be 700° C. at 12 miles, or about 20 kilos. from the surface. Iron ceases to be magnetic between 700° C. and 800° C., and it seems, therefore, fair to assume that below this depth magnetic matter does not exist. Whether this be so or not, it is necessary to suppose that at some given distance from the surface the earth may be regarded as magnetically uniform. In selecting such a distance for purposes of calculation the relations between the magnetic properties of iron and temperature afford perhaps the most trustworthy guide.

Let then a level surface be regarded as homogeneous. It may be called the *magnetic floor*. Let matter, magnetised by the earth's induction, be supposed to be placed upon it, and let all calculations be based on the hypothesis that the permeabilities with which we have to deal differ but little from unity.

The disturbance produced by the magnetic mass will be the same whether the magnetic floor is magnetic or non-magnetic; for the same coating of south hemisphere magnetism, which in the latter case will represent the effects of the earth's induction on the lower surface of the mass, will represent the modification it produces in the magnetisation of the floor if the latter is magnetic.

The disturbances which have to be explained are of two kinds, viz., those in which a very great range of vertical force disturbance occurs within a limited area, and those in which a moderately high value of vertical disturbance occurs over a large area.

In all cases the effects of the upper and lower surfaces of the disturbing masses will be opposed, and the force observed will be the same if the disturbance is produced by similar masses, the distances of which from the attracting point are proportional to their linear dimensions. Even supposing, therefore, the shape and the magnetic properties of the mass are known, we learn nothing as to its proximity to the surface from the mere magnitude of the disturbing force at a single station.

On the other hand, the absence of vertical disturbing force does not necessarily prove that no magnetic matter exists between the magnetic floor and the point of observation. The upper and lower surfaces of a plate, of which the horizontal dimensions are very large compared with the distance of either from the surface, would produce equal and opposite effects over the central parts.

On approaching the plate from a distance the vertical disturbing force would increase near the edge, and then die out as the centre was approached, the phenomena so far corresponding with those which occur when the observer crosses an underground ridge of magnetic rocks. The two cases, however, could be distinguished by the fact that the direction of the horizontal forces would be the same on both sides of the line of maximum vertical force if the disturbance were due to the edge of a plate, but different if it were produced by a magnetic ridge.

I now propose, therefore (1), to describe the distribution of vertical force disturbance over England and Wales; (2) to show that the presence beneath the surface of rocks which possessed *in situ* the same magnetic properties as basic rocks on the surface would produce disturbing forces of the same order as those which are actually observed.

In discussing the first point, it will be convenient to measure disturbances or departures of the magnetic force from its normal calculated value as terms of 0·00001 C.G.S. or 0·0001 metric unit, which may be regarded as the *unit* of *disturbing force*. Vertical disturbing forces are positive when they urge a north-seeking pole downwards.

It is fully explained in the published account of the magnetic survey that vertical force disturbances are measured from an arbitrary datum, and that there is no proof that this is uniform all over the kingdom.

The largest area of positive disturbance occurs in the east and south-east of England. It extends from the English Channel to the north of Yorkshire, *i.e.*, about 230 miles north and south, and in parts it is about 110 miles wide.

It is, however, deeply cut into by a narrow gulf-like region of negative disturbance in the Midlands, and by others in Kent, Sussex, and South Lincolnshire.

Taking this into account, and also the fact that in the north it is very narrow, it is fair to assume that it may be approximately represented by a rectangular figure 180 miles long and 108 miles wide. The regions of negative vertical force which bound it are relatively small, say from 15 to about 50 miles wide in the west, while to the north they are wider.

To the east and south the sea prevents our determining the exact limits of the district. It may, however, be taken as a rough but

adequate representation of the actual state of things to assume that the district has no magnetic matter to the north and south; but it is separated from other similar regions on the east and west by a non-magnetic trench 36 miles wide.

The difficulty in accounting for a high average difference of vertical disturbing force increases with the size of the district, as has already been explained. We are, therefore, choosing regions which afford the most severe test of the theory that the disturbing forces are due to the magnetism induced in iron-bearing rocks. If we confine our attention to a line crossing the two trenches and the plateau half-way between their northern and southern boundaries, no important error will be introduced if we consider the length of the trenches and plateau to be indefinitely extended north and south. The values obtained are, however, affected by the assumptions we make as to the magnetic character of the ground east and west of the district under consideration. The first hypothesis made is that the external boundaries of the trench are narrow plateaux, and that beyond them there is no magnetic matter. Afterwards, we will suppose that they extend to infinity at a uniform height above the magnetic floor. We may also, at first, assume that the edges of the magnetic masses are vertical, and that they are magnetised by the vertical component of the earth's field only. It will be convenient to express all distances in kilometres, and, as above stated, all disturbing forces in terms of 0·00001 C.G.S. or 0·0001 metric unit.

The mean values of all the positive and all the negative vertical force disturbances in England and Wales, recorded by Dr. Thorpe and myself, are +59 and −80 respectively, giving an average range of about 140.

A difference of this order can be obtained if we suppose that the slabs of magnetic matter are 16 kilom. (or about 10 miles) thick, and that thus the upper surfaces are 4 kilom. (or about $2\frac{1}{2}$ miles) from the surface of the earth.

The components of the attraction exerted on a point by a plane rectangular plate consisting of an uniform layer of attracting matter of density σ may be expressed as follows:—

Let planes be drawn through the point perpendicular to the plate and parallel to its edges. Let the points in which these meet the edges or edges produced of the rectangle be joined to the point, and let these make angles ϕ_2 and ϕ_1, θ_2 and θ_1 with the normal.

Then the components perpendicular to the plate and parallel to it and to the plane of the ϕ's are—

$$\sigma\{\sin^{-1}(\sin\phi_2\sin\theta_2)-\sin^{-1}(\sin\phi_2\sin\theta_1)-\sin^{-1}(\sin\phi_1\sin\theta_2)$$
$$+\sin^{-1}(\sin\phi_1\sin\theta_1)\},$$

and
$$\sigma\{\log_e(\cos\phi_1\sin\theta_2+\sqrt{1-\sin^2\theta_2\sin^2\phi_1})$$
$$-\log_e(\cos\phi_2\sin\theta_2+\sqrt{1-\sin^2\theta_2\sin^2\phi_2})$$
$$-\log_e(\cos\phi_1\sin\theta_1+\sqrt{1-\sin^2\theta_1\sin^2\phi_1})$$
$$+\log_e(\cos\phi_2\sin\theta_1+\sqrt{1-\sin^2\theta_1\sin^2\phi_2})\}.$$

The component parallel to the plane of the rectangle and to that of the θ's is obtained from the last by writing ϕ for θ and *vice versâ*.

In the particular case for which $\phi_1=\theta_1=0$ and $\theta_2=\pi/2$, the expressions reduce to

$$\sigma\phi_2 \text{ and } -\sigma\log_e\cos\phi_2=\sigma\log_e\sec\phi_2.$$

In the case under consideration these must be doubled, as the plate is supposed to extend both north and south of the point.

If we assume the magnetic susceptibility of the mass to be 0·0016, which is about the mean value of the Mull basic rocks, and take the vertical component of the earth's field as 0·46 C.G.S. units, we get—

$$2\sigma=2\times0{\cdot}46\times0{\cdot}0016=0{\cdot}00147,$$

so that if ϕ_2 is expressed in degrees

$$2\sigma\phi_2=0{\cdot}0000257\,\phi_2.$$

also
$$2\sigma\log_e\sec\phi_2=0{\cdot}00338\log_{10}\sec\phi_2.$$

Hence the disturbance produced is $2{\cdot}57\,\phi_2$ and $338\log_{10}\sec\phi_2$ units of disturbing force respectively.

By means of these expressions we may calculate the attractions exerted at points on the surface 10 kilom. apart by horizontal plates of magnetic "matter" of opposite kinds 60 kilom. wide, and of indefinite length at depths of 4 and 20 kilom. respectively.

Subtracting the numbers given by the lower from those deduced from the upper plate, we get the resultant vertical and horizontal forces due to the opposite magnetisation of the upper and lower surfaces.

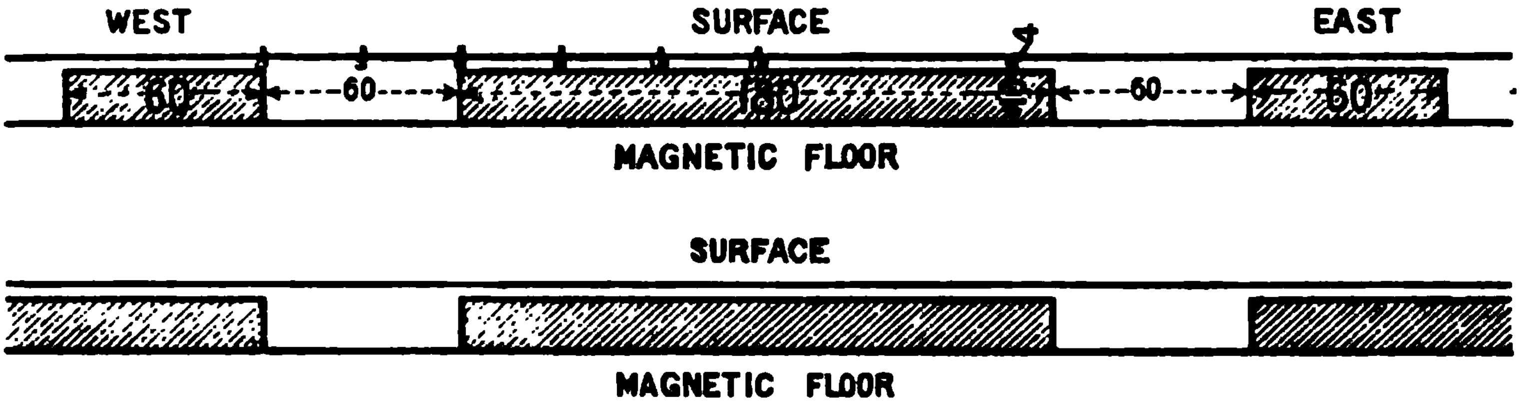
The minor plateaux are supposed to consist of two such plates, the principal plateaux of three plates side by side.

The resultant force at any point is obtained by adding, algebraically, the components due to each mass.

Fig. 3 represents the two cases which have been submitted to calculation. They differ only in that the magnetic matter outside the central plateau is supposed, in the one case, to be limited, and in the other, to be unlimited, in an east and west direction.

The forces are determined for points on the surface 10 kilom. apart.

FIG. 3.



Thus 0 being over the outer edge of the trench, 3 is over the middle of the trench, 6 over the edge of the plateau, and 15 over its centre. The forces are expressed in terms of units of disturbing force or 0·00001 C.G.S. unit. Horizontal forces are positive which urge a north-seeking pole towards the central plateau. Vertical forces are positive which act downwards as above stated.

	Case I.		Case II.	
	Vertical.	Horizontal.	Vertical.	Horizontal.
0	21	−209	− 41	−223
1	− 98	− 80	−155	− 93
2	− 90	− 23	−144	− 34
3	− 87	9	−136	0
4	− 94	41	−144	34
5	−108	99	−155	93
6	6	227	− 41	223
7	117	102	72	99
8	101	49	55	46
9	81	20	40	20
10	73	12	30	14
11	66	8	24	9
12	62	4	20	5
13	59	1	17	3
14	57	0	16	1
15	56	0	16	0
Mean positive.	+64	..	+ 32	..
„ negative	−95	..	−117	..
„ range ..	159	..	149	..

Other disturbances may be imagined which give similar results. If the central plateau stood alone, surrounded by non-magnetic matter, the mean range, as calculated above between the vertical forces over it and those observed within the 60 kilom. (36 miles) of its edge, would be 127. If the edge of the valley, instead of being vertical, were vertical for 10 kilom. only, and then sloped upwards with an inclination of 1 in 10, the range would be 107.

Although, therefore, the method of testing the theory is necessarily very rough, it is nevertheless evident that the range of the vertical forces over a mass of magnetic matter of almost the same area as the region of positive vertical force disturbance in England, and within 60 kilom. of its edge, might agree fairly with the facts if the edges of the mass were steep, and if its magnetic properties were the same as those of the Mull basaltic rocks. The observed range is 140, and widely differing hypotheses give calculated values between 107 and 159.

It is not, however, sufficient to account for the differences of vertical force over large areas. Within there are smaller but still large districts in which the vertical force disturbance considerably exceeds the mean, and the explanation of the phenomena by rock magnetism would be imperfect if the calculated forces were insufficient to explain the characters of these. It might easily be that, to produce an average vertical force disturbance equal to that which obtains over the whole plateau, it was necessary to bring the upper surface so near to the surface of the earth that the remaining depth was insufficient to account for the additional forces in play on the area of greater disturbance within it.

The largest area of very high vertical force which has at present been mapped in detail lies in Lincolnshire and South Yorkshire.

The following table exhibits the names of stations within it, and the vertical force disturbance at each:—

Sutton-on-Derwent ..	229	Thorne	230
Market Weighton	389	Butterwick	293
Howden	245	Brigg............. ...	251
Doncaster	225	Market Rasen	277

Mean of all, 267. Excluding three highest, 236.

If these places are connected by straight lines, an irregular figure of eight is produced, the area of which is about 425 square miles, or 1100 square kilom.

It is, of course, unlikely that the stations are all close to the edge of the attracting mass, but, on the other hand, it may be deeply cut into by valleys or regions of less disturbance. The latter hypothesis, if correct, would make it easier to explain the high forces, whereas an extension of the district beyond the limits actually observed makes any such explanation more difficult. The distance between the most northerly and most southerly stations (Sutton-on-Derwent and Market Rasen) is 45 miles, or 75 kilom. The distance east and

west between Doncaster and Brigg, which are near the centre of the southern loop of the figure of eight is about 27 miles, or 45 kilom.

It may, therefore, fairly be assumed that the order of the forces within the district will be the same as that of those over a rectangle 60 kilom. in length and 30 in breadth. The linear dimensions of such a figure are somewhat less than the extreme length and breadth of the district, but its area is 1·6 times greater. Let us then superpose upon the plateau a rectangular mass, 60 kilom. long by 30 broad, and 3·5 deep, the upper surface of which is, therefore, at a depth of 0·5 kilom. or 1638 feet.

The vertical disturbing forces due to this mass along a line passing over its centre and parallel to its longer edge, are as follows:—

Distances are expressed in kilometres.

Distance.		Force.
Middle	0	76
	10	79
	20	98
	25	123
Edge	30	37
	40	−34
	50	− 9
	60	− 4

Taking the mean of the numbers at the edge and at a distance of 5 kilom. from it as applying to the district between them, the mean of the positive vertical disturbances is 83.

If this minor mass were placed on the larger one, so that their longer edges were parallel, and that the median line of the smaller mass was 20 kilom. from the edge of the larger one, thus corresponding with the position 8 in fig. 3, p. 527, and if, lastly, the distribution of magnetic matter were as is shown in that figure, then the mean vertical disturbance along the median line of the small mass would be $101 + 83 = 184$.

In comparing the observed and calculated values it is convenient to take the mean force in the negative districts as a datum; so that the disturbance as calculated is $95 + 184 = 279$, and as observed $80 + 236 = 316$.

This latter number is obtained by excluding the high values at Market Weighton, Butterwick-on-Trent, and Market Rasen, but these can easily be imitated by placing small masses on the upper surface.

A plate, 5 kilom. square and 0·25 kilom. thick, approaching the surface to within 0·25 kilom., or 820 feet, would increase the force by

40. A mass of magnetic matter in the shape of a frustum of a cone, of which the vertex was in the surface, and the angle between the generating and median lines $\tan^{-1}\sqrt{2}$, and of which the upper plane surface was 450 feet from the surface of the earth, would exert at the vertex a vertical force 231, thus bringing the total calculated disturbance up to $279 + 231 = 510$, as against $384 + 80 = 464$, observed at Market Weighton. Such a cone gives a maximum effect; but it must be remembered that, although no magnetic rocks exist near the surface at Market Weighton, yet the fact that the older rocks approach the surface near that town led Professor Judd to advise its being selected as a station, and it may well be that the underlying magnetic masses rise steeply in its neighbourhood.

It is a little difficult to summarise an argument of this kind, but if we start from the near mean negative disturbance in the "valley" as a datum, and distinguish the other stations as being over an underground plateau, mountain, and peak respectively, we get the following results:—

Station.	Depth of nearest point of magnetic matter.	Mean vertical disturbance in terms of 0·00001 C.G.S.	
		Observed.	Calculated.
Valley....	No large mass of magnetic matter above the magnetic floor, *i.e.*, within 20 kilom., or 12 miles, of the surface	0	0
Plateau...	4 kilom. = 2·5 miles....................	140	159
Mountain .	0·5 kilom. = 1640 feet....................	318	279
Peak.... .	0·137 kilom. = 450 feet....................	464	510 max.

It is thus possible to imagine a distribution of magnetic matter at depths between 450 feet and 12 miles, and of no greater permeability than Mull basalt, the mere presence of which in the earth's magnetic field would produce vertical disturbing forces of the same order as those actually observed.

The maximum calculated horizontal forces are larger than any of those which have been measured in England or Wales. Case I (see Table, p. 527) has been adopted as the basis of calculation, and the horizontal disturbing force at the edge of the plateau is thus 227. Forces of this magnitude are found in Scotland, but the largest value in England is 124 at Melton Mowbray.

At the middle of the edge of the smaller mass, on the side on which

its horizontal attraction strengthens that due to the plateau as a whole, the resultant force would be about 450, a value which is surpassed only by such stations as Canna and Soa, which are islands in the West of Scotland.

While, then, in order to account for the high vertical forces, we have been obliged to make favourable assumptions as to the shape of the masses, as to the position of the smaller on the larger mass, and as to the magnetic properties of the rocks, we now find that these lead to possible values of the horizontal disturbing force considerably greater than any which have been measured.

The most obvious explanation of the discrepancy is the assumed verticality of the sides of the magnetic masses. If the sides of the valley slope for half the height at an inclination of one in ten, as described above (p. 527), the largest horizontal force due to the plateau at any of the points for which the calculation has been made is 123, as against 227 when the sides are vertical. It must, however, be remembered that in this case the mean range of vertical forces is reduced from 159 to 107. It is not, therefore, convenient to assume that the slopes are very gradual.

The supposition that the main mass is surrounded by magnetic matter of less permeability than itself reduces the horizontal forces, but also reduces the vertical forces below the observed values.

An irregular outline, on the other hand, might tend to increase the range of the vertical force at points near the edge, while it would diminish the horizontal forces.

It appears on the whole, however, that the gentle slopes or gradual changes of permeability which would reduce the horizontal forces to the observed values, would give vertical forces about one-third too small.

I do not think that this can be considered an unsatisfactory result, but I will defer comment upon it in order to turn to another point. So far, we have been discussing districts of widespread disturbance. In Scotland the forces are more localised, but more intense. The most rapid change of vertical force disturbance which has been measured is in the Southern Hebrides, where it varies from -736 at Loch Boisdale to $+369$ at Bernera, which is only 20 miles distant.

Dr. Thorpe and I have proved ('Roy. Soc. Proc.,' vol. 47) that a very intense centre of disturbance exists in this neighbourhood, and it is remarkable as being near the highly magnetic rocks of Mull, but, as its effects appear to be more far reaching than those of that island, it is legitimate to assume that it is produced by rocks of exceptional magnetic power. An equal range of force could, however, be produced by the mean permeability of basalt in the west of Scotland, *i.e.*, 0·00271.

If, as before, we take the attracting mass to be the frustum of a

cone, of which the vertex is in the surface, the vertical angle is $2 \tan^{-1} \sqrt{2}$, and the upper and lower faces are y_0 and y kilom. from the surface respectively, the force exerted at the vertex is

$$2\pi\sigma \times 0{\cdot}887 \log_{10} (y/y_0)$$

where σ is the density on the horizontal surface.

If $y = 20$ and $y_0 = 0{\cdot}5$ kilom., this gives a vertical disturbing force of 1110. By thus assuming a favourable form for the rock mass, it is possible to account for the force by means of ordinary basalt, which nowhere approaches the surface nearer than a depth of 500 metres, or about 1600 feet.

Some difficulty may be felt about Ireland, over a large part of which the vertical force disturbance is negative.

If this be regarded as a real upward force, it could only (on the hypothesis under consideration) exist in the neighbourhood of magnetic matter, which would probably cause more widespread positive disturbance than has been registered.

I am inclined to account for this by a shift in the datum from which the disturbances are measured. If the calculated vertical forces are all 0·00100 C.G.S. unit too large in Ireland, the vertical disturbance would be nearly everywhere positive. An error of this sort would be accounted for by an error of 0·00040 C.G.S. unit in the *horizontal force*, and this again would correspond to a displacement of the lines of equal horizontal force through 6 miles. If, however, the error was due to an unfavourable combination of inaccuracies, both in the lines of equal dip and equal horizontal force, this displacement of the lines might be reduced, so that, on the whole, it is not impossible that the datum in Ireland may be 100 units of vertical force disturbance different from that in England. If this is so, and if the change is gradual across England, the difference between the means of the negative and positive vertical disturbance would be diminished, and the calculated would be brought into closer accord with the observed range of vertical forces.

As the Malvern Hills have been so carefully studied, it seemed worth while to see whether the observed deflections of the needle towards the hills could be accounted for by their magnetic nature.

It is, however, at once evident that the problem is beset with difficulties. The permeability of the range appears to be different in different parts, and a mean value will not give accurate results. The visible mass of igneous rock, supposed to be of mean permeability, is certainly insufficient to account for the observed effects, and if we attempt to base calculations on assumed underground extensions of the mass, they are of course founded on pure hypothesis.

The easiest way of attacking the problem is to calculate the sum of

the two attractions exerted on opposite sides of the hills towards them.

Observations have been made at four stations, two (Great Malvern and Malvern Wells) on the eastern, and two (Mathon and Colwall Green) on the western, side of the hills. Great Malvern and Mathon are near the north end of the range, and the sum of the two attractions is 0·00243 C.G.S. unit of force. Malvern Wells and Colwall Green are near the middle of the range, and the sum of the attractions is 0·00118. It will thus be seen that at what would, *primâ facie*, have appeared the most favourable position the forces are smaller, but the result accords with the fact that the specimens collected at the north end of the range contained the largest quantity of magnetite. Sections across the range have been published by the Geological Survey, and from these it seems that on the eastern side the wall of igneous rock is nearly vertical, while on the western side it slopes more gradually under the sedimentary rocks, and if continued as far from the range as our stations (about a mile and a quarter, or 2 kilom.), it must be at a depth of 2000 feet, or, say, 600 metres. Assuming, then, that our stations were near the edge of the horizontal extension of the igneous rocks, and that the latter extend north and south for 8 miles (13 kilom.), I find that the sum of the attractions on opposite sides of the range near its centre, and at points as distant from it as our stations were, is 0·00291, 0·00154, or 0·00067, according as we assume the mean permeability to be—

(1) That of the three most favourable rock specimens;
(2) That of all the specimens which possessed measurable permeabilities;
(3) That of all the specimens obtained.

If these are compared with the observed forces, viz., 0·00243 and 0·00118, it is seen that, while they are of the right order of magnitude, an exact numerical agreement could only be obtained if we supposed the mean permeability of the range to be somewhat greater than that of its surface, as judged by the specimens collected by Mr. Highfield.

The results may, I think, be considered to support the view that the igneous rock extends laterally at a moderate depth from the surface, at least on the western side of the hills, to a distance of a mile or a mile and a quarter, but probably not much more, from the axis of the range.

In conclusion, then, I am anxious that the purport of the calculations above described should not be misunderstood. In particular, I do not attempt to specify the depth at which magnetic matter exists where none appears on the surface, in the east of England or elsewhere. But, in spite of this uncertainty about every detail, the

investigation, I think, supplies for the first time a definite answer to the enquiry whether the mere presence in the earth's magnetic field of concealed magnetic rocks, such as those which exist on the surface, would suffice to account for the observed local or regional magnetic disturbances in districts where the superficial deposits are non-magnetic.

The question is not answered by pointing to the large disturbances produced close to basic rocks, for these may be, and probably often are, due to permanent magnetism. But, as this is very irregularly distributed in the surface rocks, we cannot regard it as a probable cause of widespread disturbance, though locally it may produce very intense effects.

It is, I think, answered in the affirmative by the above discussion.

In gauging the value of the answer, it must be remembered that only one of the various constants involved has been at our disposal, and that it would not have been possible to imitate the observed results by assigning to it appropriate values, whatever arbitrary assumption had been made as to the others.

Thus, in the simple case of a rectangular magnetic slab, the average vertical force produced over it in virtue of the earth's inductions, depends on five variables, viz., its length and breadth, the distance of its upper and lower horizontal surfaces from the surface of the earth, and its permeability. Of these, the first two have been defined by the observed magnitude of the areas of high vertical force. The depth of the lower surface has not been fixed to suit the exigencies of the argument, but deduced from the temperature at which iron ceases to be magnetic. The permeability is at most that given by experiment on the specimens of basic rocks from the west of Scotland. Only one disposable constant remained, viz., the depth of the upper surface, and by shifting this we can do no more than raise the average disturbing force to a certain maximum, which might have been much less than the observed disturbances. As a matter of fact, however, by choosing suitable depths we are able to obtain forces of exactly the right order of magnitude. With the constants chosen there appears to be some difficulty in obtaining correct relative values of the vertical and horizontal disturbing forces, though even here the order of their magnitude is unaffected. This is exactly the kind of difficulty which might almost certainly be expected in calculations based only on more or less probable assumptions. It will be diminished by any change which increases the intensity of the calculated forces, and there are several possible causes which might produce such an effect.

Thus no account has been taken of the increase in the permeability of magnetite with temperature. This is certainly a *vera causa*, and will tend to bring observation and calculation into closer agreement.

Again, the high specific gravity of the earth, as compared with that

of the surface rocks, makes it probable that the interior is largely metallic, and it is possible that, even at depths less than that assigned to the magnetic floor, iron may exist in large quantities with the very high permeability it possesses at high temperatures.

Lastly, the basic rocks of Mull give a much smaller mean value of κ than those of Skye and Ardnamurchan. If the underground rocks were as permeable as these, all difficulty would vanish.

On the other hand, there are some considerations which point to the opposite direction. Thus, Professor Judd informs me that he leans to the opinion that in igneous magmas subject to the great pressures which obtain at considerable depths iron tends to form silicates rather than magnetic oxide. If this is so, the permeability of the surface rocks may be a maximum rather than a minimum, unless native iron exists in large quantity at great depths. The effect of pressure on permeability is an unknown factor, which might support or weaken the argument.

Points such as these are, however, outside the scope of this paper. But, though it is obviously unwise to be dogmatic on a question which is still surrounded by difficulties, I think that the result of the present enquiry is much in favour of the rock-magnetism theory of regional magnetic disturbances.

OBITUARY NOTICES OF FELLOWS DECEASED.

Rudolf Julius Emmanuel Clausius was born on the 2nd January, 1822, in Cöslin, in Pomerania. He was the sixth son of the Rev. C. E. G. Clausius, D.D., Councillor of the Royal Government School Board, and later, Superintendent in Ueckermünde.

After the completion of his studies in the Gymnasium in Stettin, he attended the University of Berlin from 1840 to 1844. In the Easter of 1844 he passed his examination "pro facultate docendi," and then finished his year of probation at the Frederic-Werder Gymnasium. Here he taught the higher classes mathematics and physics. In the autumn of 1846 he entered Boeck's Royal Seminary for higher students. On the 15th July, 1848, he took his degree in Halle "eximia cum laude" (subject of dissertation—"De iis Atmosphæræ Particulis quibus Lumen reflectitur"). On the 25th September, 1850, he was invited to be Professor of Physics in the Royal Artillery and Engineering School at Berlin. On the 18th December he delivered his inaugural lecture as *docent* at the University of Berlin ("De Motu Corporum rotantium in Aëre resistente"). On the 29th August, 1855, he was called to be Ordinary Professor in the Polytechnicum in Zurich, and also at the same time in the University of Zurich. In 1867 he was appointed Professor in the University of Würzburg, and in 1869 he went to Bonn, where he fulfilled his duties till the day of his death, the 24th August, 1888.

While at Zurich he married, on the 13th November, 1859, Adelheid Rimpam, of Brunswick. They had six children, of whom two daughters and two sons are alive. His wife died in 1875, and he married again, in 1886, Sophie Sack, of Essen, by whom he had one son.

His brother, Herr Robert Clausius, thus writes of the character of this great man:—

"I had often the opportunity of admiring the rare energy and clearness with which, in a small study and with limited means, he untiringly pursued his great scientific aims. A chief characteristic was his sincerity and fidelity. Every kind of exaggeration was opposed to his nature. Even as a youth all intimate with him learnt to esteem his reliability and truthfulness. In the Gymnasium and in all circumstances of his later life the greatest confidence and trust

were placed in him. His judgment, which was guided by the strongest feeling of rectitude, was highly valued. Another important trait was his unbending and firm faithfulness to duty, to which he was true in all affairs of life up to his death. Even on his last bed of sickness he held an examination. He was the best and most affectionate of fathers, fully entering into the joys of his children. He himself supervised the schoolwork of his children. He was simple and natural in his intercourse and possessed of a rare modesty. He was never tired of sacrificing himself in cases of necessity, and though immersed in abstract studies he always kept a warm heart for everything human. He was a great, noble-minded, good man, whom all who knew more intimately have loved and esteemed, not only on account of his scientific celebrity but especially on account of his noble, manly qualities.

"His burning patriotism did not permit him to stay idly at home during the war 1870–1. He undertook the leadership of an ambulance corps, which he formed of Bonn students. At the great battles of Vionville and Gravelotte he helped to carry the wounded from the battle and to lessen their sufferings. For his services in this campaign he received the Iron Cross. A contusion in his leg which he received on the field of battle caused him great pain for many years and often necessitated his driving to his lectures. On the doctor's advice, at the age of 56 he learnt to ride, and became an excellent horseman. Riding proved very beneficial to his health."

One of his sons thus writes of him :—

"The principal trait in my father's character was, without doubt, the splendid truthfulness of his nature. In his deeds and words he never could tolerate anything ambiguous, and particularly as regards himself he cherished no self-deception. For that reason he never suffered from the discovery of the motives of his actions; from thence sprang his thoroughly noble nature as well as his great modesty and the delight which he always felt, and was never too proud to express, at the recognition of his work; from thence his dislike to all smattering and superficiality in which he suspected some untruthfulness. Another remarkable characteristic was the uncommon one of seeing only the best side of his neighbours. He hardly noticed their faults, and when he did he had not the least inclination to cheap mockery. The general impression which he received of people was formed from the more or less strong development of their good qualities and was little dimmed by the presence of this or that defect. His hearty, pleasant, always thoroughly genuine address towards everyone was the result of this trait. Only when he discovered untruthfulness did he take a deep aversion."

Clausius received the following offers of posts :—

In 1858 to the Polytechnicon of Carlsruhe; in 1862 to the

Polytechnicum of Brunswick; in 1866 to Vienna; in 1868 to Munich; in 1871 to Strasburg; in 1883 to Göttingen.

He received the following orders and titles :—

When Professor in Würzburg he was appointed Court Councillor; in 1868, as Professor in Bonn, he was appointed Privy Councillor; in 1870 he received the Huygens Medal; in 1871 the Iron Cross; in 1873 the Order of the Crown, 3rd Class; in 1879 the Order of the Red Eagle, 3rd Class, and the Copley Medal: in 1881 he was named Officer of the Legion of Honour; in 1882 he was appointed Doctor of Medicine *honoris causâ* in Würzburg; in 1883 he received the Poncelet prize; in 1884 the Order of the Prussian Crown, 2nd Class; in 1885 the Bavarian Maximilian Order. During 1884–5 he was Rector of the University of Bonn, and for six months was Curator of this University. He took part in the academic education of Prince William, afterwards Emperor of Germany. In 1887 he was invited to be one of the Curators of the New Imperial Physical and Technical Institution. In 1888, at the Investiture of the Order for Art and Science, he was appointed Hereditary Knight.

He received the following honours from Learned Societies :—

In 1857 Hon. Mem. Harlem; in 1859 Hon. Mem. of the Engineers of Scotland and Corresp. Mem. Erlangen; in 1865 Corresp. Mem. of the Institute of France; in 1866 Hon. Mem. Frankfurt-am-Main, and Hon. Mem. Dublin and Corresp. Mem. Göttingen; in 1868 Royal Soc. Lond.; in 1869 Hon. Mem. Nat. Hist. Soc. Zurich; in 1871 Elected Mem. Munich; in 1872 Elected Mem. Pest; in 1873 Corresp. Mem. Bologna and Elected Hon. Mem. Boston; in 1875 Hon. Mem. Civil Engin. Lond., Elected Corresp. Mem. Vienna, and Mem. Brussels; in 1876 Corresp. Mem. Berne, Society of Arts Geneva, and Hon. Mem. Nat. Hist. Soc. Basel; in 1877 Elected Mem. Göttingen; in 1878 Elected Mem. Stockholm, Elected Mem. Naples and St. Petersburg; in 1879 Mem. Halle Natural Hist. Soc. and Elected Mem. Ling.; in 1882 Corresp. Mem. Milan, Corresp. Mem. Turin, and Hon. Mem. Mech. Eng. New York; in 1883 Elected Mem. Washington, Hon. Mem. Erlangen, and Foundation Mem. Internat. Soc. of Electricians; in 1884 Corresp. Mem. Cherbourg and Corresp. Mem. Lucca; Hon. Mem. Manchester, Mem. Amsterdam; in 1887 Hon. Mem. Brunswick, Hon. Mem. Hamburg, Ordinary Mem. Upsala; in 1888 Mem. Edin.

In his scientific work Clausius investigated the general mechanism of Nature rather than particular applications of the principles he discovered; he was constructively synthetical rather than analytical. It is remarkable how he was led by dim previsions, as when his theory of gases influenced his views on heat when his gas theory was yet quite imperfect; his greatest work is grouped round his insight into molecular structure. The other great branch of his work is connected with electromagnetic theory. As in the theory of heat, he

worked from the theory of matter to the theory of the steam engine, so in electromagnetism, he worked from the theory of electromagnetic actions to the theory of its industrial application to dynamos.

Clausius' first publications were concerned with the action of atmospheric dust on sunlight. This seems to have directed his thoughts to molecular physics, which was indirectly the foundation of his greatest work. Thermodynamics was the region he explored, and his exploration was guided by his insight into molecular physics. When Clausius was beginning his independent activity the investigations of Rumford, Davy, Mayer, Joule, and Helmholtz had conclusively shown that heat could be produced from work, while the thermodynamic speculations of Carnot, founded upon the assumed indestructibility of caloric, were receiving every day additional confirmation. There was an obvious difficulty here of which Carnot himself was doubtless aware. Of this difficulty, in 1849, Sir William Thomson writes, that if we abandon Carnot's fundamental axiom "we meet with innumerable other difficulties insuperable without further investigation and an entire reconstruction of the theory of heat from its foundations. It is, in reality, to experiment that we must look, either for a verification of Carnot's axiom and an explanation of the difficulty we have been considering, or for an entirely new basis of the theory of heat."

It was at this juncture that Clausius, without waiting for additional experiments, read, in the Berlin Academy on the 18th February, 1850, his paper, "Ueber die bewegende Kraft der Wärme, und die Gesetze, welche sich daraus für die Wärmelehre selbst ableiten lassen."

Carnot had assumed that a heat engine gave out the same heat at the lower temperature as it took in at the higher, and founded his theory on this assumption and upon the impossibility of perpetual motion. Clausius, in the first place, emphasised that the heat given out must be less than the heat taken in by an amount equivalent to the work done, that this was required by the First Law of Thermodynamics, the equivalence of Heat and Work. Thus modified, Carnot's theorems could no longer rely for their proof on the impossibility of perpetual motion, and it was Clausius' great discovery to found Thermodynamics upon the New Second Law of Thermodynamics, "That heat tends to flow of itself from hot to cold bodies." On these foundations Clausius raised again the Theory of Thermodynamics, and thenceforward there was no serious doubt as to its security. Several different ways of stating the Second Law of Thermodynamics have been advocated, and objections have been raised to each of them. Of these things Clerk Maxwell writes that Clausius "first stated the principle of Carnot in a manner consistent with the true theory of heat." Of the varieties of statement he writes: "By comparing together these statements the student will be able to

make himself master of the facts which they embody, an acquisition which will be of much greater importance to him than any form of words on which a demonstration may be more or less compactly constructed."

There can be no doubt that Clausius was the first to throw a clear light upon the then dark and doubtful foundations of Carnot's theorem. Sir William Thomson writes of this in 1851, "the merit of first establishing the proposition upon correct principles is entirely due to Clausius."

As Professor Willard Gibbs says, "Rankin was attacking the problem in his own way with one of those marvellous creations of the imagination of which it is so difficult to estimate the precise value." The question of the amount of mechanical effect to be derived from heat, he further says, "was completely answered, on its theoretic side, in the memoir of Clausius, and the science of thermodynamics came into existence." "It might be said, at any time, since the publication of that memoir, that the foundations of the science were secure, its definitions clear, and its boundaries distinct." To Clausius then be the honour of making a science of Thermodynamics.

Clausius' subsequent work in this line consists essentially in working out the results of the law he discovered, and in investigating its foundations on general dynamical principles applied to molecular physics. In working out the results of his law he explored in two directions. He applied his discovery to work out the theory of the steam engine, and of many known phenomena, and also to discover properties of matter revealed by his analysis. This latter line is contiguous with his exploration of the dynamical foundations of the theory of heat. His analysis revealed the existence of entropy as a property of matter, a property for which mankind has no sense, such as exists for feeling temperature, and which consequently escapes attention, and is most difficult of apprehension; so difficult, indeed, that although fundamentally as important as temperature in the theory of steam engines, its existence is ignored by all except the very foremost amongst those who study the working of steam engines. Clausius has left little to be done in the theory of heat engines except to work out, in the lines he has laid down, the details that experiments may prove to be most important. Clausius applied his theory to investigate the laws of specific and latent heat, of saturated steam, of the relations of heat and electricity in conductors, in thermopiles, and in electrolytes, and to the laws of radiant heat. He showed that radiant heat was no exception to the law that heat flows of itself from hot to cold bodies, and so proved the futility of the ingenious suggestion that the death of the universe by the degradation of energy might be avoided by the reconcentration of heat radiations by reflection from the confines of the ether. He showed

that the radiating power of black bodies in various media was proportional to the square of the refractive index of the medium, which involves a corresponding law of radiation of electromagnetic energy.

In all these applications his attention was constantly directed to the underlying molecular motions which explained the phenomena on dynamical principles. It was in this connexion that he investigated the dynamical foundations of the Second Law of Thermodynamics and the molecular theory of gases and of electrolysis.

In quite an early investigation he had been dominated by the conception that heat in a body can be considered as separated into two parts, one the kinetic energy of atoms, and the other the potential energy of forces between atoms. His later investigations were elaborations of these conceptions by the application of statistical methods, and by mathematical analysis of the highest order. He showed that the heat in a body could be expressed as the product of two factors, one proportional to the mean kinetic energy of the atoms, and the other depending on the mass, velocity, and period of their motions. These factors may be identified with temperature and entropy, and so furnish a dynamical basis for the theory of heat. Involved in these investigations was the Theorem of the Virial, which is so important in the dynamics of stationary motion. His theory is in accordance with much that we know, though it neglects radiation, and forces between molecules depending on their motions and positions, which may be systematically different before and after collision. With the ether among the molecules it is almost impossible but that some such forces exist, while the success of dynamical theories that neglect them seems to show that their effect cannot be very great.

While Clausius was elaborating these general results, he attacked the simpler case of the molecular theory of gases. That the properties of gases were in some way due to the motions of their molecules was a hypothesis as old at least as Bernoulli, but it was Clausius who raised it to the rank of a theory, and he has been described by Clerk Maxwell as the principal founder of the science. He showed how Boyle's and Dalton's laws followed from the theory, and how they were approximate; he proved the existence of intramolecular energy, and the necessity for Avcgadro's law, and for the equality of mean energy of translation; and, insisting on the necessity for two atoms in a molecule, hastened the advent of the change in atomic weights which chemists were adopting; he investigated the length of the mean free path of a molecule, and the rates of diffusion and conductivity of heat in gases.

In connexion with the molecular theory of matter, Clausius had as early as 1851 investigated some of the laws of evaporation, and

had shown that the law of corresponding temperatures that Groshans modified from Dalton involved the law that at any given pressure the latent heat of vaporisation per unit volume of many substances was the same. After Van der Waals' memorable paper on the continuity of the liquid and gaseous states, Clausius published an elaborate investigation of this subject, in which he developed the application of a rather complicated formula, and showed that it represented the experiments throughout an enormous range with wonderful accuracy. There is a measurable departure from the law in calculating the compressibility of the liquids. To facilitate the application of this formula, Clausius invented and calculated the values of a special transcendental.

Electricity and magnetism attracted Clausius' attention from time to time, at first in connexion with heat and molecular physics, and afterwards with reference to the theory of electrokinetic actions. In 1858 he developed the theory that the molecules in electrolytes are continually interchanging atoms, and that the effect of electric force is to direct the interchange and not to cause it. It seems, however, possible that as synchronous systems near the solar system might break it up, while asynchronous ones might not, so a polarisation of the atomic motions in a liquid might result in a proportionate breaking up of the molecules which before this introduced regularity were stable. In reply to Hittorff's objection that gases should obey Ohm's law, Clausius answered that there were too few molecules. This can hardly be considered conclusive in presence of electrolytic conduction in very dilute aqueous solutions. His theory, however, explains almost all the known facts of electrolysis, and has been extended by others to explain many other phenomena with most remarkable success. He investigated electric osmosis, and hints that it is produced by electric forces due to charges over the surfaces of the pores in the diaphragm. He remarked that the resistance of pure metals is proportional to their absolute temperature.

His electrokinetic theory, founded on a theory of action at a distance between electrical elements moving in conductors, led to the conclusion that the action between the elements must depend on their absolute velocity and not on their relative velocity. This practically postulates a medium with reference to which the elements move, and by which the actions are propagated.

With the great development of electrodynamics as a machine for applying energy to do man's work, Clausius repeated his exploration of the theory of the steam engine, that great machine for applying heat energy to do work, and investigated on broad principles the theory of dynamos. Some of his work in this direction is superseded by the rapid development of the science and its applications, but his insight into the problem is evidenced by his having been one

of the first to notice hysteresis, which he describes as the forces resisting magnetisation being like friction.

It is to be regretted that in his electromagnetic theory Clausius was led by the algebraic methods of Weber rather than by the geometrical insight of Faraday, or by some mechanical theory, such as directed his steps in thermodynamics. If his constructive genius had been here well directed, there might now exist a satisfactory theory of electromagnetic action; he might have founded the theory of ether as well as of gas; he had genius enough to do it.

Though not himself an experimentalist, he valued and was eminently able to criticise and use the results of experiment. He had that clear grasp of natural phenomena which leads to a right interpretation of them, and that concrete practical conception of them that leads to a continual reference back of the interpretation to experimental numerical verification. He was a noble example of the spirit that devotes itself to directly benefiting mankind, and that does not waste time on petty elaborations of pretty problems. He was in the highest sense practical, his work is eternal, and his memory will live as long as mankind reveres its benefactors.

G. F. F. G.

Sir WILLIAM GULL died on the 28th day of January, 1890, at his house in Brook Street, in his 74th year. His was one of the many distinguished names which refute the imputation of dulness upon the Eastern counties. He was born in the north-east corner of Essex, at one of the many villages which retain the old English name of Thorpe. While he was still a child his father died, and he was dependent for his education upon his mother's character and his own exertions. While teaching in the village school, he attracted the attention of the late Mr. Benjamin Harrison, for many years the Treasurer of Guy's Hospital, and the wise ruler of its Medical School. Thorpe-le-Soken lay in the midst of the Essex estates of the Hospital, and the Treasurer performed all the duties of a good landlord. Telling the friendless youth, "If you will help yourself I will help you," he brought him up to London, and gave him employment in transcribing Museum catalogues, and other clerical work in the Hospital counting-house. While thus employed Gull matriculated at the University of London in 1838, and by the Treasurer's influence was admitted to attendance on the courses of lectures at Guy's. His industry and talents procured him an honourable degree in 1841. Dr. Quain, who graduated the year before, Sir Edmund Parkes, who passed in the same year, and Sir Alfred Garrod and Dr. George Johnson, who followed in 1842, all became like him Fellows of this Society, and all, with the exception of the lamented Parkes, survive him.

Before he proceeded to the M.D. degree, at which he obtained the Gold Medal, he had been appointed by the Treasurer as a Supernumerary Resident Medical Officer, with special charge of the lunatic patients, who at that time occupied a department of the Hospital. For about nine years he seems to have been employed every day, and almost all day long, in the medical wards, acquiring by constant and close observation that familiarity with every variety of disease which afterwards stood him in such good stead.

In 1843 he received his first appointment in the Medical School, that of Lecturer on Natural Philosophy, and three years later wa transferred to the Chair of Physiology.

For three years, from 1847 to 1849, he was also Professor of Physiology in the Royal Institution.

In 1848 he married, and in 1851 he was appointed Assistant Physician to Guy's Hospital. In 1858 he was promoted to be full Physician, and for several years lectured on medicine with the late Dr. Owen Rees, also a Fellow of this Society, who died only a few months before Sir William Gull, after an illness of almost exactly the same nature and the same duration.

Very early in his career Dr. Gull obtained a large practice. His natural sagacity, his unusually large experience, his knowledge of disease and his knowledge of mankind, all combined in his favour. His striking physiognomy, his gravity and self-possession, his power of sympathy, and the well-chosen words in which he delivered his opinions fitted him to gain the confidence of his patients. But, apart from his remarkable practical skill, he showed a true scientific spirit in dealing with disease. He was unwearied in patient investigation, sparing no time where it was needful, although he wisely would refuse to trouble himself or his patient with minute collateral enquiries when once the essential nature of the case was apparent. He was never satisfied with the commonplace explanations which too often do duty for science, or the commonplace prescriptions which too often do duty for treatment. He had so thorough a knowledge of medicine that he would have been successful in spite of every personal defect, and his skill in dealing with patients was so consummate that he would have been successful if he had been ignorant of his profession.

In 1871 a severe attack of enteric fever from which the Prince of Wales suffered brought Dr. Gull's name into public notice, and his services were acknowledged by a Baronetcy, with a grant of Arms.

After his retirement from hospital practice, Sir William Gull did not allow his enormous consulting practice to absorb his energies or his interest. Few men have practised a lucrative profession with less eagerness to grasp at its pecuniary rewards. He kept up the honourable standard of generosity to poor patients which has been handed

down from Mead and Heberden; and, with a liberality which is less common, he showed no jealousy of younger men sharing in his good fortune. Not one, but five or six of those who were rising in the profession, owed much of their success to his help. His name was familiar to the public, and his practice increased beyond his power to overtake his engagements. It is supposed that for several years he made as large an income as that of Sir Astley Cooper in 1814. But the fortune which he left at his death was, it is well known, only in part the result of his professional earnings.

Sir William Gull was independent in his relations with his patients, and was always glad to devolve as much of his work as he could upon younger colleagues. He also found time for the punctual performance of the public duties which his position imposed upon him. On the Council of this Society, on the Senate of the University of London, on the General Medical Council, and on that of the Association for the Advancement of Medicine by Research, he was a constant and influential attendant. He went through the various offices of Lecturer, Councillor, Orator, and Censor in the College of Physicians, and was generally looked to as its next President at the time of his last illness.

This was in the long vacation of 1887—just before his return to Brook Street to resume his work. He was attacked with apoplexy while walking in his grounds in Perthshire; and, though the attack soon passed off, its significance was apparent, and to none more so than to the patient. He gave up practice and spent the next two years partly at Eastbourne and partly near Reigate. Repeated returns of his disorder, with varying severity, were watched by him with intelligent interest and full appreciation of their meaning. His intellectual faculties were spared him to the last, and nothing became him better than the fortitude and clear-sightedness with which he watched the process of decay, the friendly relations which he kept up or renewed with old colleagues or pupils or rivals, and the resigned submission with which he awaited the end. This came in a sudden and painless attack on the morning of the 28th of January in the present year. A few days later he was buried in the churchyard of his native village, beside his father and mother's grave, attended by representatives of his University, of the Royal Colleges of Physicians and Surgeons, of Guy's Hospital, and by a crowd of colleagues, friends, and pupils.

His striking appearance, his slow movements and deep voice, with his grave, thoughtful, but kind and sympathising manner, admirably fitted Sir William Gull to be an adviser and a helper in time of need. Few physicians have so completely acquired and retained the confidence of their patients.

As a hospital teacher, his method was minute and elaborate in investigation, clear and comprehensive in diagnosis, sagacious in fore-

cast, cautious and modest in treatment. While not inferior in accurate physical diagnosis to those who made this the whole of their art, he would never follow the routine system of diagnosis by successive exclusions, which, however useful to a junior student, is of very limited application at the bedside. In really obscure cases, where definite physical signs failed to indicate the nature, or even the existence of the disease, few physicians have approached the almost intuitive sagacity of Sir William Gull.

He was often supposed to be entirely sceptical as to treatment; and this scandal, though unjust, was no doubt fostered by some too sweeping words of contempt for polypharmacy and unverified therapeutical dogmas. In reality, his treatment was extremely careful, scrupulous, and minute, judicious in its aim, ingenious in its devices, and occupied with those little matters of detail which make so much difference to the comfort of the patient, and the success of any plan of therapeutic. His use of drugs was scarcely more sparing than the practice of other scientific physicians. In suitable cases no one wielded such powerful weapons as opium, mercury, arsenic, and digitalis with more confidence and skill. He used iron and quinine and acids, and bitters and purgatives much as others use them, but with a full recognition of the limited power of drugs, the natural limitations of acute disease, and the inevitable progress of degeneration and decay.

His eminence as a clinical teacher, a lecturer, and a practitioner was so conspicuous, that his merits as a pathologist and observer have scarcely received the credit they deserve. It is not too much to say, that he takes rank among the five or six Englishmen of his generation who advanced medical knowledge at more than one important point. He never published a book, and his papers are scattered in the volumes of the 'Guy's Hospital Reports' and in the Transactions of Societies. In one of the earliest papers which he published (in conjunction with Dr. Addison) was given almost the first notice and the first complete description of the remarkable disease of the skin known as Xanthelasma. His papers on Intermittent Hæmaturia, on the Treatment of Rheumatic Fever, on the Treatment of Tænia, and on Fictitious Urticaria were all accurate and original contributions to clinical knowledge. But his articles on Paraplegia in the 'Guy's Reports,' and on Abscess of the Brain in 'Reynolds' System of Medicine,' established pathological facts of primary importance. The paper which he published, in conjunction with Dr. Sutton, upon "Arterio-capillary Fibrosis" deals with a subject of great difficulty which is still, and probably will long remain, under investigation. It undoubtedly gave a new point of view—and a most instructive one—to the student of chronic Bright's disease. His account of Anorexia Nervosa was also marked by keenness of discrimination and breadth

of view, and it anticipated much that has since been written. The last important paper he published was on a Cretinoid Condition in Adult Women. It announced in succinct and modest fashion the discovery of a remarkable form of disease, up to that time quite unrecognised, and the accuracy of its description as well as the justice of its pathological views have since been entirely confirmed.

Foremost among clinical physicians of his time, and eminent among the pathologists, remarkable for the breadth of his views, the liberality of his conduct, and the native force of his character, Sir William Gull occupied an eminent position during his life, and has left a record of work which worthily answers to his great reputation.

P. H. P. S.

STEPHEN JOSEPH PERRY was born in London on August 26th, 1833. His early education was received at Gifford Hall School, from whence he went to the Benedictine College at Douay, and then to Rome to study for the priesthood. Resolving to join the Society of Jesus, he returned to England, and on November 14th, 1853, entered on his novitiate, and after two years went to France, where he stayed for a year, returning to Stonyhurst to enter upon a course of mental philosophy and physical science. His great ability in mathematical studies decided his superiors in the Order to allow him to devote himself specially to this branch of study, with the result that he stood sixth on the Mathematical Honours List of London University in 1858, during which year he attended lectures by De Morgan at London, and by M. Bertrand and others at Paris. On his return to Stonyhurst in 1860 he was appointed Professor of Mathematics and Director of the Observatory, which posts he filled for three years, when he went to St. Beuno's College, North Wales, to complete his theological studies previous to his ordination as priest, in 1866.

In 1868 he resumed his work at Stonyhurst as Professor and Director of the Observatory, and continued there until his death, only leaving the College to take part in scientific expeditions.

Previous to the appointment of Father Perry to the directorship, the work done at Stonyhurst Observatory had been chiefly meteorological and magnetic; it was selected as a first-class meteorological station in 1866.

In 1867 the astronomical department was much extended, more powerful instruments were acquired, and the work undertaken very much increased. Spectroscopy was commenced at Stonyhurst in 1870, and since then has formed a large portion of the astronomical work carried on there. In 1872 regular observations of the phenomena of Jupiter's satellites were begun with the 8-inch Troughton and Simms equatorial, and with one considerable break, 1875—77, have been continued up to the present time.

In 1875 Father Perry instituted a regular series of drawings of the markings on the Sun from a projected image of 10½ inches diameter, formed on a drawing board attached to the telescope. This work was supplemented by spectroscopic examination and measurement of the chromosphere and prominences, and after 1882 by systematic spectroscopic observations of Sun spots. In addition to the researches in solar physics, in connection with which the name of Father Perry has become so well known, the ordinary work of an observatory has been most diligently carried on at Stonyhurst, the results of which for the most part have been recorded in the publications of the Royal Astronomical Society.

It was not, however, in his work in the Observatory and College that Father Perry found an outlet for his untiring energy. His services were always at the disposal of scientific societies, and he took part in nearly every astronomical expedition of his time, and although a martyr to sea sickness, he was always ready and willing to undertake any voyage and undergo any hardship in the cause of the science he loved.

His first important scientific work was done in 1868, when, in conjunction with Father Sidgreaves, S.J., he made a magnetic survey of the West of France. With the Stonyhurst instruments complete sets of observations were made at fifteen stations during that year, and the work being continued the year following observations were obtained at nineteen other positions in the East of France, and thus a valuable and reliable magnetic survey of that country was completed. In 1871, assisted by Mr. W. Carlisle, of Stonyhurst, Father Perry carried out a similar work in Belgium, where the disturbing effect of the coal measures was very clearly indicated in the results obtained, which, together with those of the survey of France, were published in the 'Philosophical Transactions of the Royal Society.'

In 1870 Father Perry was appointed chief of the Solar Eclipse Expedition to Cadiz, his work being principally spectroscopic; and in 1874 he was selected by the then Astronomer Royal, Sir G. B. Airy, to take command of the Transit of Venus Expedition to Kerguelen Island. On September 20th he and his party left the Cape of Good Hope for Kerguelen Island in H.M.S. "Volage," on a journey attended by great difficulties and dangers, owing to the fearful weather usually prevailing off Kerguelen Island. Intense as must have been the suffering of Father Perry during the voyage, and severe the hardships of a nineteen weeks' sojourn on the "Island of Desolation," as he called it, all were cheerfully borne, every personal consideration being sacrificed to the astronomical interests of the Expedition. The observations of this transit were imperfect on account of haze, and the necessity of determining the exact longitude of their station prolonged the visit of Father Perry and his companions until February

26th, 1875, when the supply of provisions ran short, and it was not deemed prudent to stay longer. The passage to Durban was very rough, the "Volage" being overtaken by a cyclone and sometimes rolling 45°. Besides the observations of the transit, magnetic observations were made at the Cape, Kerguelen, Bombay, Aden, Port Said, Malta, Palermo, Rome, Naples, Florence, and Moncalieri; and on his return to England Father Perry gave an account of his work in a lecture at the Royal Institution.

In 1882 he again took charge of a Transit of Venus Expedition, this time being stationed in Madagascar, and accompanied by Father Sidgreaves and Mr. Carlisle, his former assistant in Belgium. The party reached their destination on October 22nd, in H.M.S. "Fawn." Again the results obtained were incomplete, wind and sand-storms interfering with the work, but some very good observations were made notwithstanding these difficulties. At the eclipse of August 29th, 1886, Father Perry accompanied the expedition to the West Indies, and was stationed at Carriacou. His spectroscopic observations were communicated to the Royal Society, and published in the 'Philosophical Transactions.'

In 1887 he was one of the observers sent by the Royal Astronomical Society, at the request of Professor Bredichin, to Pogost, on the Volga; but the sky was cloudy and no results were obtained.

Father Perry was again, in 1889, placed in command of an expedition sent by the Royal Astronomical Society, to the Isles de Salut, off French Cayenne, to photograph the corona during the eclipse of December 22nd. He and his assistant, Mr. Rooney, reached Barbadoes on November 26th, and arrived at his station on December 7th, after a trying voyage from Barbadoes, in H.M.S. "Comus," during which Father Perry suffered severely. Every assistance was afforded by the French Commander, and by the officers and crews of H.M.SS. "Comus" and "Forward" during the preliminary arrangements, but the necessary adjustments required exposure to the unhealthy night air, and this so told upon Father Perry, in his already weakened state, that on the day before the eclipse he was very ill indeed from dysentery; so ill that it was feared that he would be unable to take part in the observations of the eclipse. However, by a tremendous effort he overcame his pain and weakness sufficiently to successfully carry out all the instructions he had received, but, exhausted by this great effort, he became dangerously ill after the excitement which had sustained him was over, and was taken on board the "Comus." On December 26th he was slightly better, but, a relapse occurred and Father Perry died at 4.30 P.M., on December 27th. His body was interred in the Catholic Cemetery at Demerara, on December 28th.

Father Perry was a very popular and able lecturer. On several occasions he has lectured at the Royal Institution, the last time, a

few months before his death, on the work at Stonyhurst in connexion with solar physics during the last ten years. He also frequently lectured on astronomical subjects in the large towns in the North of England.

At the time of his departure from England, on the expedition that resulted in his death, Father Perry was contemplating a full discussion of all the work carried on at Stonyhurst, a work which would undoubtedly have formed a most valuable contribution to our knowledge of solar physics.

Father Perry was elected a Fellow of this Society in 1874, and in November, 1889, was made a member of the Council for the ensuing year. He became a Fellow of the Royal Astronomical Society in 1869, and was also a member of the Council of that Society. He was a Fellow of the Royal Meteorological Society and the Physical Society of London, and was President of the Liverpool Astronomical Society at the time of his death.

Of foreign Societies, he was a Member of the Accademia dei Nuovi Lincei, of the Société Scientifique de Bruxelles, and of the Société Géographique d'Anvers. He has been for several years a member of the Committee on Solar Physics appointed by the Lords of the Committee of Council on Education, and of the British Association Committee for comparing and reducing Magnetic Observations. In 1887 and 1889 Father Perry assisted at the International Congresses on Astronomical Photography held at Paris. In 1886 he received the degree of D.Sc. from the Royal University of Ireland.

In the death of Father Perry science has sustained a loss universally felt and most deeply regretted by all labourers in that branch to which his heroic self-sacrifice and never-failing energy have contributed so much.
A. A. C.

WILLIAM KITCHEN PARKER, born at his father's farm at Dogsthorpe, near Peterborough, June 23, 1823, died suddenly, of syncope of the heart, whilst visiting his second son, Professor W. Newton Parker, at Cardiff, July 3, 1890. Whilst cheerfully talking of late discoveries and future work in his favourite biological pursuits, he ceased to breathe. Accustomed to outdoor life, he was a true lover of nature from the first; the forms, habits, and voices of birds, especially, he knew at an early age. Village schooling at Dogsthorpe and Werrington, and a short period at Peterborough Grammar School, prepared him for an apprenticeship, at fifteen years of age, to Mr. Woodroffe, chemist and druggist, at Stamford; and three years afterwards he was apprenticed to Mr. Costal, medical practitioner, at Market-Overton. At Stamford, studying botany earnestly, he collected and named more than 500 species of plants. The fauna also of the fen lands attracted his attention—in Borough Fen, Thorpe Fen, Whittlesea, Deeping,

and Crowland. Both when living under his father's roof, and in his holidays afterwards, he kept many pet animals, and dissected whatever he could get, including a donkey and many birds. Of the latter he prepared skeletons; and of these he made large drawings at Market-Overton, which of late years he had some thought of publishing as an atlas of the osteology of birds.

Without the advantages of a university education, and with none of those aids to learning afforded by the science schools of the present day, he owed all the knowledge which he acquired to an intense love of nature, prompting and developing a taste for original research; and this, in spite of many obstacles, he assiduously cultivated to the last.

In December, 1844, he came to London, and entered Charing-Cross Hospital as a medical student. Having had an introduction to Dr. Todd, he was cordially received by him and encouraged to work in his physiological laboratory at King's College; and for a time he was prosector at Dr. Todd's lectures. He qualified as Licentiate of the Society of Apothecaries in 1849, and commenced to practice at Tachbrook Street, Pimlico. Soon afterwards he married Miss Elizabeth Jeffery. His wife's patient calmness under all difficulties and trials was a true blessing to a man of Mr. Parker's excitable temperament and indifferent health; and her unselfish life and wide-spread influence for good are well known in and beyond the family circle. Unfortunately, he was left a widower about four months before his death. He left three daughters and four sons. Of the latter, one is a Fellow of the Royal Society, and Professor of Zoology and Comparative Anatomy in the University of Otago, New Zealand; the second is Professor of Biology in the University College at Cardiff, South Wales; the third is an able draughtsman and lithographer; and the fourth has taken his diplomas of L.R.C.P. and M.R.C.S.

Mr. Parker had a good father, courteous and gentle by nature, conscientious, and earnest in business, who had worked hard to be able to give even his youngest son, Mr. W. K. Parker, "a start in life." From his placid and thoughtful mother he probably inherited much of his love of reading and readiness to learn.

Always energetic, in spite of ill-health, Mr. Parker enthusiastically carried on his medical work and his natural-history studies, especially in the microscopical structure of animal and vegetable tissues. Polyzoa and Foraminifera, collected on a visit to Bognor, and from among sponge-sands and Oriental sea-shells, especially engaged his attention. Having sorted, mounted, and drawn numbers of these Microzoa, he was induced, about 1856, by his friends W. Crawford Williamson and T. Rupert Jones to work at the Foraminifera systematically. His paper on the *Miliolitidæ* of the Indian Seas ('Transact. Microscopical Society,' 1858), and a joint paper (with T. R. Jones)

on the Foraminifera of the Norwegian Coast ('Annals and Mag. Nat. Hist.,' 1857) resulted; and the latter formed the basis of a memoir on the "Arctic and North-Atlantic Foraminifera" ('Phil. Trans.,' 1865). With T. Rupert Jones, and afterwards with W. B. Carpenter and H. B. Brady, Mr. Parker, down to 1873, described and illustrated many groups and species of Foraminifera, recent and fossil (see Sherborn's recent 'Bibliography of Foraminifera,' for these papers and memoirs), thereby establishing more accurately a natural classification of these Protozoa, determining their bathymetrical conditions, and therefore their value in geology. The important share which he took in the preparation of Dr. Carpenter's 'Introduction to the Study of the Foraminifera,' 4to, published by the Ray Society in 1862, is acknowledged in the preface of that handsome volume. That he did not neglect anatomical research is shown by memoirs in the Proceedings and Transactions of the Linnean, Zoological, and Microscopical Societies on the osteology (chiefly cranial) and systematic position of *Balæniceps* (1860), *Pterocles* (1862), *Palamedea* (1863), Gallinaceous Birds and Tinamous (1862 and 1866), Kagu (1864 and 1869), Parrot (1865), Ostriches (1866), *Microglossa* (1865), Common Fowl (1869), Eel ('Nature,' 1871), skull of Frog (1871), of Crow (1872), Salmon, Tit, Sparrow-hawk, Thrushes, Sturgeon, Pig, and Ægithognathous Birds (1873), Woodpecker and Passeres (1875). In the meantime the Ray Society had brought out his valuable 'Monograph on the Structure and Development of the Shoulder-girdle and Sternum in the Vertebrata' (1868); and his Presidential addresses to the Royal Microscopical Society (1872, 1873), and notes on the *Archæopteryx* (1864) and the fossil Bird bones from the Zebbug Cave, Malta (1865 and 1862), had been published. Subsequently the Royal Society's Transactions contained his abundantly illustrated memoirs on the skull of the Batrachia (1878 and 1880), of the Urodelous Amphibia (1877), the Common Snake (1878), Sturgeon (1882), *Lepidosteus* (1882), Edentata (1886), Insectivora (1886), and his elaborate memoir on the development of the wing of the Common Fowl (1888). In the 'Reports of the "Challenger"' is his memoir on the Green Turtle (1880). Those on the Cypselidæ ('Zoologist,' 1889), on *Tarsipes* (Dundee, 1889), the Duck and the Auk (Dublin, 1890), Gallinaceous Birds (for the Linnean Society), and the Hoatzin (*Opisthocomus cristatus*) for the Zoological Society, are his last works.

In former times a skull was regarded as little more than a dry, symmetrical, bony structure; or, if it were the cartilaginous brain-case of a shark, it was to most a mere dried museum specimen. When, however, the gradations of the elements of the skull, from embryonic beginnings, were traced until their mutual relations and their homologues in other Vertebrates were established, light was

thrown on the wonderful completeness of organic uniformity and singleness of design. How such studies can be carried on both by minute dissection and the modern art of parallel slicing, and not by one method alone, is to be gathered from his teaching.

As a draughtsman, Mr. Parker particularly excelled, and the value of his numerous memoirs was greatly enhanced by the excellence of the plates, the figures in which were drawn by himself. The article on the Anatomy of Birds in the 'Encyclopædia Britannica' also bears evidence to his industry and knowledge.

No man can have worked harder at science, in the intervals of professional duties, than he did, and it is scarcely surprising that the short intervals which he allowed himself for rest affected his health and compelled him to limit his practice. Like a true naturalist, however, he allowed his love of science to triumph over any desire for worldly gain, and it was well known to his friends that some of his best scientific work was accomplished during actual physical suffering, furnishing him, as he would say, with a pleasant distraction from his ailments.

In 1864 he was elected into the Zoological Society without the usual fees: and soon afterwards the Linnean Society paid him the same high compliment.

Mr. Parker was elected a Fellow of the Royal Society in 1865, and in the year following he received a Royal Medal for his comprehensive, exact, and useful researches in the developmental osteology, or embryonal morphology, of Vertebrates. Some few years afterwards the Royal Society made him an annual grant to aid in the prosecution of his studies; and, when that was discontinued, a pension from the Crown was graciously and appropriately awarded to him. A generous friend, belonging to a well-known Wesleyan family, more than once presented £100 towards the cost of some of the numerous plates illustrating his grand memoirs in the 'Philosophical Transactions.' He was elected Fellow of King's College, London, in 1875. In 1873 he had received the diploma as Member of the Royal College of Surgeons, and was appointed Hunterian Professor of Comparative Anatomy and Physiology, Professor Flower being invalided for a time; and aferwards both held the Professorship conjointly. His earnestness and wide views were well appreciated, opening up the modern aspect of comparative anatomy, and showing that both in Man and the Lower Vertebrates the wonderful structural development of their bony framework should be studied in a strictly morphological rather than a teleological method, and that its stages and resultant forms could be regarded only in the Darwinian aspect.

These lectures, given in abstract in the medical journals, became the basis of his 'Morphology of the Skull,' in editing which Mr. G. T. Bettany ably assisted him; and in a less scientific book, 'On

Mammalian Descent,' another friend (Miss Arabella Buckley, now Mrs. Fisher) similarly helped him. In the latter work, his own usual style frequently predominates, full of metaphor and quaint allusions, originating in his imaginative and indeed poetic mind, fully impregnated with ideas and expressions frequent in his favourite and much-read books—Shakespeare, Bacon, Milton, some of the old divines, and, above all, the old English Bible.

Separating himself from the trammels of foregone conclusions, and from the formulated, but imperfect, misleading conceptions of some of his predecessors in biology, whom he left for the teaching of Rathke, Gegenbaur, and Huxley, Professor W. K. Parker earnestly inculcated the necessity of single-sighted research, and the following up of any unbiassed elucidations, to whatever natural conclusion they may lead. Simple and firm in Christian faith, resolute in scientific research, he felt free from dread of any real collision between science and religion. He insisted that "our proper work is not that of straining our too feeble faculties at system-building, but humble and patient attention to what nature herself teaches, comparing actual things with actual" ('Proc. Zool. Soc.,' 1864); and in his "Shoulder-girdle, &c.," p. 2, he writes: "Then, in the times to come, when we have 'prepared our work without, and made it fit for ourselves in the field,' we shall be able to build a 'system of anatomy' which shall truly represent Nature, and not be a mere reflection of the mind of some one of her talented observers."

Again, at p. 225, in illustration of some results of his work, he says:—"The first instance I have given of the Shoulder-girdle (in the Skate) may be compared to a clay model in its first stage, or to the heavy oaken furniture of our forefathers, that 'stood pond'rous and fixed by its own massy weight.' As we ascend the Vertebrate scale, the mass becomes more elegant, more subdivided, and more metamorphosed, until, in the Bird class and among the Mammals, these parts form the framework of limbs than which nothing can be imagined more agile or more apt. So also, as it regards the sternum; at first a mere outcropping of the feebly developed costal arches in the Amphibia, it becomes the key-stone of perfect arches in the true Reptile; then the fulcrum of the exquisitely constructed organs of flight in the Bird; and, lastly, forms the mobile front-wall of the heaving chest of the highest Vertebrate."

Professor W. K. Parker was a Fellow of the Royal, Linnean, Zoological, and Royal Microscopical Societies; Fellow of King's College, London; Honorary Member of the Philosophical Society of Cambridge, and the Medical and Chirurgical Society. He was also a Member of the Imperial Society of Naturalists of Moscow, and Corresponding Member of the Imperial Geological Institute of Vienna, and the Academy of Natural Sciences of Philadelphia. In 1885 he

received from the Royal College of Physicians the Bayly Medal, "Ob physiologiam feliciter excultam."

In conversations shortly before his death, he often spoke of looking forward throughout his life-time (alas! how quickly shortened!) to continued application of all the energy he could devote to his useful work—at once a consolation to him and a duty.

He has well expressed his own view on biological pursuits, at p. 363 of the 'Morphology of the Skull':—"The study of animal morphology leads to continually grander and more reverent views of creation and of a Creator. Each fresh advance shows us further fields for conquest, and at the same time deepens the conviction that, while results and secondary operations may be discovered by human intelligence, 'no man can find out the work that God maketh from the beginning to the end.' We live as in a twilight of knowledge, charged with revelations of order and beauty; we steadfastly look for a perfect light, which shall reveal perfect order and beauty."

An unworldly seeker after truth, and loved by all who knew him for his uprightness, modesty, unselfishness, and generosity to fellow-workers, always helping young inquirers with specimens and information, he is lost to sight as a friend and father, but lives in the minds of his fellow-workers, of those whom he so freely taught, and of his bereaved relatives, as a great and good man, whose beneficent influence will ever be felt in a wide-spreading and advancing science by thoughtful and appreciative men.

T. R. J. and J. E. H.

Robert William Mylne, who died in July, 1890, aged 74, was for thirty years a Fellow of the Royal Society, to which also his father and his grandfather belonged. He was descended from a family eminent for several generations in architecture and engineering, his grandfather, Robert Mylne, F.R.S., and his father, William Chadwell Mylne, F.R.S., both having been engineers of eminence, and both attached to the New River Company.

Robert William Mylne was closely associated with his father in the active management of the New River Company. He was also for some years engineer to the Limerick Water Company, and was frequently consulted upon wells and water-supply both by the Government and private companies, and at one period of his life often gave scientific evidence on Water Bills before the House of Commons. He obtained a good water-supply for one of the sunk forts in the sea off Portsmouth, and was employed on the well at Tilbury and other fortifications.

In the department of geology he was, perhaps, best known, and his geological map of London and the neighbourhood, a work of immense labour and expense, was long a standard authority amongst

scientific men. He also prepared many other maps, which are less widely known.

He always devoted much time to the study of archæology and antiquarian matters, and was preparing an elaborate work on the architectural antiquities of Eastern Scotland at the time of his death. He was thirty years a Governor of Bridewell and Bethlehem Hospitals. He was also a Fellow of the Society of Antiquaries of London, and of Scotland, a Fellow of the Geological Society of England, and also of France, and a Member of the Smeatonian Society of Civil Engineers. He was also indirectly connected with the water works at Frankfort and Buda Pesth, and the Canal du Midi, in Southern France.

W. B. D

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ERRATA.

Page 378,	line 30,	*for*	"action"	*read*	"reaction."
„ 382	„ 17	„	"heated"	„	"treated."
„ 387	„ 10	„	"with acids"	„	"into acids."

END OF FORTY-EIGHTH VOLUME.

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